5078

5078

The Basis and Structure of Knowledge

THE BASIS AND STRUCTURE OF KNOWLEDGE

W. H. WERKMEISTER
PROFESSOR OF PHILOSOPHY
UNIVERSITY OF
NEBRASKA

HARPER & BROTHERS PUBLISHERS
NEW YORK AND LONDON

To Lucyle - now as always

THE BASIS AND STRUCTURE OF KNOWLEDGE
Copyright, 1948, by W. H. Werkmeister
PRINTED IN THE UNITED STATES OF AMERICA
All rights in this book are reserved. No part of the book may be reproduced in any manner whatsoever without written permission except in the case of brief quotations embodied in critical articles and reviews. For information address Harper & Brothers

B-X

CONTENTS

INTRODUCTION

In 1930 the late Moritz Schlick published an article significantly entitled *Die Wende der Philosophie*—"The Turn in Philosophy." In this article the founder of "logical positivism" advanced the thesis that "we find ourselves in the midst of an absolutely final turn in philosophy." In particular, Schlick contended, the new school of thought has done away with all "traditional problems of epistemology" and has eliminated from philosophical discussion all questions concerning "the validity and the limitations of cognition."[1]

For a number of years Schlick's thesis has been echoed and re-echoed in the writings of "logical positivists." Lately, however, it has found fewer adherents, and the fascination it once had for many thinkers is fading away. Since the days of Schlick, philosophical analysis has made considerable progress and has led to a repudiation of many of the basic contentions of the positivists. It has brought us anew face to face with problems and questions which Schlick and his early followers regarded as intolerable in philosophical discussions. We may no longer be disturbed by "traditional" problems of epistemology, and we may consider traditional "solutions" of such problems as meaningless anachronisms; but the fact remains that we, in our own time, are confronted once more with all the fundamental issues pertaining to an analysis of the basis and structure of knowledge, and that we must find our own solution to problems which gave impetus to the philosophies of Descartes, Hume, and Kant, and which will arise again and again as long as men earnestly seek and value knowledge.

The present "crisis" in philosophy—if such we want to call it—is a direct result of the astonishing advances made in the last one hundred years in mathematics and the physical sciences. The invention in the early nineteenth century of non-Euclidean geometries paved the way for a clear understanding of the nature and function of the postulational method and led to a broader and more far-reaching use of this method outside the field of mathematics.

[1] M. Schlick, "Die Wende der Philosophie," *Erkenntnis*, I (1930-1931), 5-7.

The negative result of the Michelson-Morley ether-drift experiment and the discovery of radioactive disintegration of chemical elements necessitated a reconsideration and general overhauling of the most basic conceptions in physics, culminating in Einstein's theory of relativity and in modern quantum mechanics. The nature of these developments was such that they corroborated and supplemented the trends which had become apparent in mathematics. In fact, most of the theoretical novelties introduced in physics were conceived in the same spirit, and depended upon the same methodological point of view which had received special recognition in the formalism of David Hilbert.

This spirit, engendered in the exact sciences, is slowly transforming the other sciences too. The principle of quantum mechanical resonance and the postulational procedure from which it derives its significance and integrative power, have already been introduced into chemistry—resulting in a complete restatement of the theoretical foundations of that science; and in the social sciences the work of Lundberg and Dodd, deficient and inadequate as it is, indicates at least the direction toward the new procedure. Throughout the whole range of scientific enterprise we thus encounter manifestations of the new approach to basic problems and of the new methodological point of view.

But are this approach and this method really "new"? Or have scientists generally only now awakened to the full realization and clear understanding of what the method and the point of view of science really mean and imply? I am convinced that the latter is the case, and that a study of the history of scientific method will bear out my contention. Scientists in all fields of inquiry have finally come to understand fully their own enterprise and are beginning to use consciously and deliberately a procedure which they have hitherto used more or less blindly as a supplement to laboratory technique and without comprehending its real power. Nevertheless, the result of the new comprehension is nothing short of an intellectual revolution and will in time affect every phase of knowledge as well as our general attitude toward problems of human existence.

The reverberations of the new spirit in science have been keenly felt in philosophy. The advances in mathematics led to a reconsideration of logic and to the development of "symbolic" logic and of various types of non-Aristotelian "logics." Moreover, the problems of meaning and verifiability have received new attention, and the general

field of semiotics is being explored as never before in the history of philosophy. Value judgments appear in a new light and, as a consequence, ethics and aesthetics may now be placed upon new foundations. A new critical attitude permeates the field of philosophy in its entirety and may lead to unexpected results. We have awakened from the dogmatic slumber of philosophical tradition and are ready to examine once again the assumptions and presuppositions upon which philosophical inquiry depends. The problems we face may be substantially the same problems which men in their strivings after knowledge have always faced; but we see these problems in a different setting, and we approach them from a new point of view. We may expect, therefore, that we can solve them in a new way.

Kant, who was thoroughly conversant with the sciences of his day, attempted to develop a philosophy which would provide a secure and adequate basis for scientific knowledge, and he tried to do this by adapting scientific analysis to the requirements of philosophy. He had caught a glimpse of the postulational procedure of science but, like all his contemporaries, he failed to comprehend its real import and meaning. Tradition was still too strong with him, and the postulational procedure had not yet demolished the fetish of a priori truths.

Furthermore, in his basic work, Kant limited his analyses so exclusively to the propositions of mathematics and physics—the only sciences actually in existence at that time—that it was impossible for him to integrate the whole of experience from a single point of view. His philosophical "system," therefore, makes the impression of being a "patchwork." Nevertheless, Kant's "Copernican revolution" was an important step in the right direction. If understood in the light of modern developments in the sciences, and if expanded so as to provide a broad interpretation of knowledge, art, and culture in general, it may still be a guidepost in our attempt at a philosophical synthesis. In this broad sense at least the present book is dominated by the spirit of Kant. Its inspiration arises from the *Critique of Pure Reason*—although the thesis here presented is not a "revival" of Kantianism and is in no way dependent upon Kant's specific formulations or solutions. As a matter of fact, I can well imagine that Kant himself might not recognize the views here set forth as "Kantian."

The basic ideas developed in the following chapters I have previously stated in a loosely knit form in a number of articles and in my book, *A Philosophy of Science*; they have, however, been substantially augmented and have been reformulated and woven into a

PART I

LANGUAGE AND MEANING

CHAPTER I

GENERAL CONSIDERATIONS

Knowledge and the Knowledge-Situation

For the purposes of this book, the word 'knowledge' is taken to mean *warranted belief*. This definition I believe to be broad enough to include all forms of knowledge which are of interest to the philosopher, and to admit a discussion of all relevant questions. It covers the knowledge of everyday life as well as that of science and philosophy, the knowledge of primitive man as well as that of the cultured scholar; and it excludes no problems of epistemology on a priori grounds. Nor does it commit us in advance to a specific philosophical "ism"; for idealists, realists, pragmatists, and positivists alike can accept it without abandoning their respective points of view. All differences of doctrine are subsequent to the above definition.

According to our definition, knowledge is *belief*. A belief is statable in propositions. Knowledge, therefore, is essentially something statable in propositions. This point is significant, for it implies a basic distinction between (1) being aware, (2) perceiving, and (3) knowing. The importance of this distinction will become apparent in a later chapter when we deal with the problem of truth. Here it is sufficient to make clear what is intended by referring briefly to some illustrative examples.

I am *aware* of a patch of red, of a sound of such and such nature, of a sweet taste, of a pain, etc.; that is to say, I am aware of sense-qualities and feelings. I *perceive* a rose, a table, a book, a mountain, a cloud, etc.; i.e., I perceive things as unitary complexes of sense-qualities. But I *know* that "this rose is red," that "this table is made of wood," that "this book is my property," that "this sound is C-sharp," etc. I know, in other words, what is statable in propositions.

Propositions as such, however, are not knowledge; for they may be false, and false propositions are not knowledge. They are not *warranted* beliefs. The statement, "There are two books on my desk," is a proposition, but it is not knowledge. Its very *claim* to be knowledge presupposes that somebody believes it or asserts it as true; and

it actually is knowledge only *if belief in it is warranted*; i.e., if there is *sufficient reason for asserting that it is true.*

If what has been said so far is at all an acceptable interpretation of the broadest aspects of the knowledge-situation, then it is evident that knowledge never exists by and for itself but always for a subject or "knower" who believes or asserts as true a given proposition or a set of propositions. And if this is so, then three distinct but related types of problems arise: (1) Who is the "knower" and how does he acquire knowledge? (2) What does or can he know? (3) When is his belief warranted?

Of these questions, the first at least must be answered in a preliminary way before we continue our discussion; for this answer will indicate the general direction of our argument. A final answer, however, cannot be given until after we have examined more closely the various issues involved in all three questions.

Until we have proof to the contrary, let us assume the "knower" to be a finite, empirical, human being—you or I. This usage of the term 'knower' is in agreement with our common-sense view; it is in complete harmony with all we learn from the sciences; and it is based upon the fact that we cannot know anything which is not in some way connected with our own experience—with your experience and with mine. In the last analysis, each and every one of us faces the problem of knowledge alone, as it arises in his own first-person experience. Only *my* first-person experience is the basis for whatever knowledge *I* attain, and only *your* first-person experience is the basis for *your* knowledge. Even so-called "public" verification rests ultimately upon first-person experiences. You observe certain "coincidences" or marks on a scale, and I observe certain "coincidences" or marks on a scale. Our agreement concerning what we have thus observed privately and each one for himself is a matter of warranted belief, and the warranty of this belief can be found only in our own respective first-person experiences—in your experience and in mine; for whatever does not prove itself as warranted belief in my own first-person experience I cannot accept as knowledge, no matter what its knowledge-claim may be. And whatever does not prove itself as warranted belief in your first-person experience you cannot accept as knowledge. Even revelation cannot impart knowledge unless that which is revealed becomes somehow a warranted belief for him to whom it has been revealed.

This does not preclude the possibility of finding criteria of warranty

which you and I and everybody else who is interested in knowledge can agree upon. It merely means that, as knower, I can know only what is somehow experienced by me—be it something of which I am directly aware, or be it something connected with that of which I am directly aware; and that I can regard my beliefs as warranted only if they conform to standards of warranty which I accept and which I find in my own experience. What others impart to me must also enter in some way into my own first-person experience if it is to become knowledge for me, and must conform to my criteria. In the end, any analysis of the knowledge-situation carries us back to our own first-person experience; and whatever transcends the sphere of this experience, without being connected with it in some way, simply cannot be known to us.

This statement is not a dogmatic assertion, nor is it derived from a Cartesian intuition. It is, rather, the inescapable conclusion drawn from an analysis of the meaning of 'meaning'—and knowledge is, of course, inseparable from meaning. In fact, meaning is the indispensable condition of knowledge, the *sine qua non* of its possibility; for statements and propositions must have meaning if they are to be believed or asserted as true. Without meaning, knowledge, as warranted belief, is impossible and non-existent.

An analysis of knowledge must therefore involve an analysis of meaning and must, preferably, begin with such an analysis.

The Meaning-Situation

Meaning arises somewhere at the level of animal life—just where, it is difficult to ascertain.

Whether or not meaning is involved when an animal comes upon proper food and devours it I leave to others to decide. But when a dog takes up the scent of a rabbit, or when a mouse smells the cheese we use for bait and comes to eat it, we may be reasonably sure that meaning of some sort is involved in the behavior of these animals.

The actual experiences of the dog or the mouse may be comparable to those of a man who returns home for dinner, smells the odors of a steak, and thereupon experiences in his digestive organs all those pleasant feelings commonly associated with the expectancy of eating a well-prepared meal. In all three cases there may have taken place only an arousal, by sensory stimuli, of specific physiological conditions of expectancy. The experience need not, and probably does not, entail the recognition of the rabbit, or the cheese, or the steak as

distinct objects or "things." The immediate sensory stimuli, nevertheless, point beyond themselves. They *indicate* something and, in this sense, they have "meaning."

The meaning encountered in these and in all similar cases is rudimentary and is but a behavioral anticipation on the part of the subject, not an "ideication" or clear recognition of the "objects" involved. Still, in situations such as these meaning has arisen in the course of evolutionary advancement; and if today 'meaning' implies more than organic or behavioral anticipation, this fact reveals the great potentialities inherent in the rudimentary beginnings but does not prove the non-existence of these beginnings themselves.

However, one decisive step beyond behavioral anticipation had to be taken before meaning in the full sense of the word, and as it is basic to knowledge, could emerge. The purely behavioral anticipation had to be supplemented and, in time, replaced by a recognition and acknowledgment of an object *as the specific referent* of the meaningful experience.

Let us consider a few typical situations in which meaning in this "referential" sense is encountered: (1) We see a dark cloud approaching rapidly and feel a sudden gust of wind; we take these occurrences to "mean" the imminence of rain. (2) While hunting, we come upon the fresh tracks of a deer; these tracks "mean" that the hunted animal is near. (3) A man glares at us and clenches his fists; we take this to "mean" that he is angry. (4) A friend points to a bookshelf; his gesture "means" that there is the book I am looking for. (5) On this stone are the special markings which we were to observe and follow on our hike through the woods; these markings "mean" that we are on the right trail. (6) The headlines of our newspaper are made up of these words, "Nazis Surrender"; this "means" that the war in Europe is over. (7) On the title page of a book we find the words, "Critique of Pure Reason, by Immanuel Kant"; this "means" that some particular person called Immanuel Kant wrote this book. (8) The central figure in a certain picture wears a crown and holds a scepter in his hand; this "means" that the person here pictured is a king or an emperor.

All of these situations—and they can be multiplied indefinitely—involve "meaning." All of them are of such a nature that we may well encounter them in actual life. Meaning, in other words, arises in actual experiential situations, not in abstract theories; and an analysis of meaning must proceed from an analysis of these concrete situations,

or must culminate in such an analysis. To this extent at least the question of meaning is a *question of fact,* not a question of "what ought to be" according to preconceived theory.

All concrete situations reveal that the experiential context in which meanings are found is a triadic relation, involving (a) a mind which interprets (b) some specific given experience or "sign" as standing for or designating (c) some (actual or imagined) object, condition, situation, or process—the "referent."

Behind this triadic relation, as its indispensable condition, and therefore as the basic presupposition of all meaning, lies the indisputable fact that *any* experiential complex can be represented in a future experience by some particular part of that complex, or by some specific experience connected with the original complex through some form of association.

Association alone, however, is not sufficient; for meaning is found only where the distinction of "sign" and "referent" is made and where, in the presence of the sign, and because of it, the mind reacts to, or *intends,* the referent. That which serves as a sign, taken by itself, does not "mean" anything. It does not point beyond itself or designate something other than itself. It simply is what it is—perhaps a cue to emotive responses or to adaptive reactions in the experiencing subject; perhaps only the sound of a voice or some marks on paper. It becomes a "sign" only when it is *intentionally taken as designating something other than itself,* when it is employed to designate a referent.

The examples of situations involving meaning show that a great variety of experiential elements may serve as signs. Often the sign is a part of the natural context—as when clouds are taken to indicate approaching rain, or when fresh tracks are taken to mean the nearness of a deer. More often, however, the sign has been specifically posited as an indicator of such and such referents; it is artifactual rather than natural.

If the sign is artifactual, it may have been agreed upon for a particular occasion and will not be used again; or it may serve as a traditional means of communication. At times, a spontaneous gesture will suffice. At other times, the sign has grown out of the common experience of a community, a tribe, or a nation, and is an integral part of our social heritage and our tribal behavior. The status and significance of the various signs is thus notably different in important respects; but all signs are *signs* only because some mind takes them

as designating something other than themselves, as referring to something which is not identical with the "event" or the "thing" which serves as the sign. This self-transcending reference which the mind imparts to the sign is the one distinctive feature of *all* signs.

It is important to note, furthermore, that the sign is seldom a copy or picture of its referent, and that our understanding of signs is completely independent of all actual or imagined semblances between them and their referents. Were it otherwise, ink marks on paper could never designate or mean abstract ideas. As it is, the pictorial character of signs, wherever found, is incidental to the meaning-situation. As signs, all gestures, markings, words, and symbols have no relation to their referents save that which comprehending minds establish between them. Whatever other relation may exist between the experiences regarded as signs and the experiences regarded as referents is irrelevant to the nature of signs *as signs*.

Sensory particulars—such as gestures, sounds, or marks on paper—have meaning only because some mind interprets them as being signs of something else. Their nature as signs is exhausted in this relation. The interpretative function of a mind is therefore indispensable to the nature of signs as signs. Books may contain thousands of words; but unless some mind interprets and "understands" them, takes them as designating something beyond themselves, all those words are only meaningless marks on paper.

The mind, however, uses signs only as vehicles—as means in and through which it comprehends the referents. The mind, therefore, is not confined within, or restricted to, the realm of signs, but transcends that realm in the sense that in every employment of a sign it "means" something other than the sensory particulars which serve as the sign. This being the case, the mind is also not dependent upon the presence of any particular sign but determines autonomously what shall serve as sign in any given situation. That we ordinarily employ conventional signs is no proof to the contrary; for this indicates only that we find it more convenient to follow established custom rather than to specify in every particular case what our sign shall be, and it does not impair in principle the autonomy of minds in their use of experiential contents as signs.

The importance of mind in the meaning-situation is strikingly evident also from the following considerations.

If someone says, "Fire," he produces certain air vibrations which, when they impinge upon a sensitive ear, result in the particular sounds 'fīr.' The sounds thus produced are unique in their physical

occurrence here and now. They possess their own specific pitch, intensity, and scale of overtones, their own harshness or softness, or whatever else may be the quality of the voice uttering them. They are thus, strictly speaking, not the *word* 'fire' at all, but only an instance of that word. If one were to say repeatedly, "Fire," or if one could induce someone else to repeat the sounds, each repetition would be a new and unique physical instance of the word 'fire' which, *as word*, is in some way different from its physical instances. If this were not so, we would not be able to utter the same word twice; nor could a spoken sound and an ink mark on paper represent the same word. Actually, the *word* 'fire' (or any other repeatable sign) is the *class* of certain specific (actual or possible) space-time events which are sufficiently similar in some respects to be recognizable as instances of the same class. To the extent to which this is true it proves that an enduring mind capable of understanding the various "utterances" as instances of the same sign is indispensable as the basis of all significative meaning.

The same conclusion must be drawn if we approach the problem from a different angle. If the sign has meaning—and if it had no meaning it would not be a sign—then it also refers to some specific referent. Here again we must face the facts squarely. The referent, too, is but a *class* of "similar" experiential contents; and the sign designates this class as a whole rather than any one of its unique instances or manifestations. The word 'fire,' for example, designates all possible fires (past, present, and future), and not only one particular fire or one unique moment of some particular fire. The use of signs thus implies that meaning is possible only on the basis of a representation of classes of experiences by classes of other experiences; and only an active and enduring mind can establish and maintain such a correlation. Meaning, therefore, is impossible without the presupposition of such a mind; and the use of signs, from its very beginning, implies a recourse to "universals."

When I use the word 'mind' to designate the persistent "knower," I do not mean "mind-stuff" or any other metaphysical entity, but simply what may be called "sign-consciousness"—the empirical fact that you and I can and do interpret signs and understand their meanings. All further interpretations of "mind" I leave, for the present, to the psychologists and to the metaphysicians. But the synthesizing activity of mind is indispensable to an understanding of meaning and, therefore, of knowledge.

Natural Languages

So far we have considered the meaning-situation only in its broadest aspects. We shall now limit in a very definite way the scope of the problems facing us.

Knowledge, so we have said, is warranted belief, and beliefs are statable in propositions. Propositions, however, when stated, are formulated linguistically. Propositions, in other words, are essentially inseparable from language. And thus it comes about that language and words attain special significance in any attempt to understand the basis and structure of knowledge. We shall therefore restrict the problem of meaning so as to include in our discussion only *linguistic signs*—words, phrases, and sentences—and shall neglect all other forms of signs and symbols as irrelevant to our main problem.

The word 'language,' as used in this book, designates any system of spoken and/or written (printed) signs or words commonly used as a means of interpreting experience and/or for purposes of communication.

The distinction between the two (related) functions of language here indicated is of considerable importance in connection with the rules of definition to be discussed later.

The component parts of a language, the words, share with all other signs the essentially referential character disclosed in all meaning-situations; but they are artifactual and clearly differentiated from signs which are part of a natural (causal) complex.

Within the realm of language a further distinction must be made. Languages which have developed gradually and have become an integral part of a tribal or national culture—such as English, Chinese, Eskimo, and Sioux—will be called *natural* languages; while languages which have been invented for specific purposes—such as the sign language of advanced mathematics or of symbolic logic—will be referred to as *constructed* languages. Of these two types of language the former will be discussed first; for natural languages are the means in and through which alone we can intelligibly discuss the nature of constructed languages and the process of their construction. And natural languages also are the primary tools for the interpretation of experience.

As far as is known, all human communities and tribes, without a single exception, have developed a natural language, while no animal species possesses more than the most rudimentary fragments of one.

Man, therefore, is in a real and distinctive sense the language-using animal. As a matter of fact, his whole cultural existence is bound up with, and depends upon, the use of language.

The actual origin of language is shrouded in the obscurity of pre-history. We can only guess at the factors involved in its formation, and at the manner of its beginning. But this much seems to be certain: Language, before becoming an instrument for the analysis of thought, was an instrument of action.

We find it to be such in the case of small children. The first words used by the child are, as a rule, not concepts in the logical sense of that word, but cues for certain forms of behavior. They do not designate objects or events, but express the feelings and desires of the child; or, if they do refer to objects, this reference is only incidental, and the words really express the emotional or volitional significance of the object for the child.

We reach the same conclusion when we study the languages and the language-habits of primitive man. Whatever may have been the origin of language—whether it arose as a battle cry or as a chant of triumph, as a mating call or as an accompaniment to bodily motion and to handwork—man gave linguistic expression to his feelings and desires long before he began to formulate his thoughts. Before they became an instrument for rational analysis, words already served as links in co-operative action, and as means to facilitate such action.

Malinowski has given us a dramatic description of a fishing expedition of primitive man which clearly shows how words and linguistic phrases—shouted as warnings, employed to solicit aid, or used to call attention to important happenings—accompany the action, determine its course, and become indispensable elements in the common enterprise.

But even the discursive speech of primitive man, the description and re-telling of victories and heroic deeds, of thrilling and terrifying experiences—although only indirectly connected with the deed or the event itself—is never merely a matter of reflection. Its function and significance in the life of the tribe, the co-operation of the "speaker" and his "chorus" of listeners, and the emotional value of the story-telling for the whole community, cannot be fully understood if they are not understood as forms of community action. The linguistic account, the telling of the story, is itself an action in and through which the events are re-created for the tribe—and re-created in the

spirit of the community and in harmony with the desires and hopes, the prejudices and fears of the collective membership of the tribe.

The words of a language which emerges from such origins are tinged with emotion; they express and arouse emotional associations which may be of far greater importance to speaker and listener than is the purely significative relation to a specific referent. And at no time in their development do natural languages ever lose all traces of their emotion-tinged origins—a fact which gives rise to numerous difficulties of interpretation but which also explains some otherwise inexplicable effects of words in our daily discourse. We shall return to this point later.

Subjective Factors in Language Formation

Wherever we look, we encounter natural languages and linguistic signification only as forms and expressions of community life. Language arises, develops, and possesses meaning only within a speech-community. It is therefore not surprising that the various languages reflect the special interests and points of view prevalent in the communities in which they arise and develop (cf. *A Philosophy of Science*, Chapter V).

Man's mind is not a *tabula rasa,* and language is not the result of impressions passively received by that mind. From the very beginning of the formation of language man has possessed at least some autonomy in his dealings with the world about him. He differentiates and evaluates; he selects, accepts, and rejects, and, in doing all this, he creates certain focal points of interest—specific points of view, from which he tries to comprehend and interpret the manifoldness of his experience.

These focal points of preference and of rejection find corresponding recognition in the forms and structures of the respective languages. That which in some way is of special significance to man; that which attracts his attention and induces him to act; that which is necessary for, or disastrous to, his existence; that which affects in some manner the welfare of his tribe; that which provides protection against his enemies or delivers him into their power; that which assures the success of the hunt or counteracts his own magic; that which pacifies the dead or cures diseases—all this is given linguistic recognition and linguistic expression.

The word, however, is never a copy of the object it designates, nor is it the mental counterpart of physical things. It is not an imprint

made by the thing for which it is a sign. A causal theory can never explain the meaning of words. The whole manner of subjective response enters into the formation of this meaning and into the development of language. The grammatical categories with all their peculiarities, exceptions, and infractions of rules—far from being impressed upon the mind by the things of the external world—are but the crystallizations of the unsystematic and purely practical attitudes which, of necessity, man has taken in his continuous struggle for existence.

The words and phrases of different languages are therefore never complete synonyms. Too many of the elements of subjective experience which led to the creation of the word have entered into its meaning. The emotional overtones of the word cannot be defined through an enumeration of the objective qualities of the thing designated. The special manner of ideation finds expression in the synthesis and in the relations upon which depends the formation of the linguistic phrase. The grammatical categories are, as it were, the linguistic coordinates with respect to which man organizes the world of his experience in conformity with his special interests.

The things for which primitive man invents names and the manner in which he draws distinctions and groups things into classes clearly reveal the subjective factors of interest and prejudice at work in the formation of language. So close at times is the connection between interests and attitudes on the one hand and linguistic forms on the other that the language of a community or tribe contains in crystallized form the accumulated world view of a people, that the language becomes a cultural index of broad implications.

The Sensory-Intuitive Basis of Natural Languages

Not only do natural languages arise from activistic and emotion-laden origins, not only are they inseparably interwoven with tribal interests, tribal aspirations, tribal hopes and fears; they are also notoriously concrete. They express in great detail matters which more advanced languages assume as understood; for, while we try to comprehend conceptually, primitive man tries to depict. Spatial relations, in particular, are represented by him with great care. Gestures are used as aids to oral descriptions.

Once man has conceived his own body as a unity persisting in the midst of a variety of changing experiences, he is ready to use this body as a key to the universe as a whole; and language reflects this

fact. Body-part words or prefixes are employed in designating spatial relations. 'Head' means *over, above*; 'mouth,' 'lips' mean *in front*; 'ear' has the meaning of *alongside*; 'neck' designates *in back, behind*; 'leg' connotes *under,* and so on.

Beyond this, the place of the speaker and the place of the person spoken to define a system of coordinates for primitive man relative to which he orders his world and interprets motion. And reference to his immediate environment may supplement the plane of coordinates defined by his own body (cf. *A Philosophy of Science*).

It must not be forgotten, however, that for primitive man space is never a neutral or purely objective scheme of things, homogeneous in all its parts. His imagination inseparably associates certain events with specific localities. The fork in the river is the place of the great battle. The rugged mountain is the home of the thunder god. The dense bush is holy ground. Primitive man experiences thus a spatial world which is completely heterogeneous in its emotional significance and, therefore, in its stratification. The sensory world provides the concrete elements in terms of which the interpretative schemes of primitive man attain distinctive meaning, but the concrete content of sensory experience alone is not decisive in fixating that meaning. The specifically synthetic nature of human minds, rather than objective space, determines the variations in experience which lead to divergent schemes for the interpretation of space and of spatial relations; and a constructive effort rather than a process of abstraction provides the basis for the various linguistic forms and structures which designate the experienced spatial scheme.

The linguistic expressions designating time reflect even more clearly the concreteness of primitive man's point of view. The peculiarly elusive nature of time, which we express adequately through manifold and variegated verbal forms, escapes primitive man almost completely. Many languages recognize only two temporal forms of the verb—a durative, designating present and future, and the preterit. The minute temporal distinctions which we recognize as present, present perfect, past, past perfect, past future, future, future perfect, and past future perfect, are impossible in most primitive languages.

In order to express concretely whatever temporal distinctions primitive man recognizes, he frequently employs space-words in referring to time or he defines temporal relations by means of such words or phrases as "commencement," "being occupied with," "to continue," "accompaniment," "completion of an act," "cessation," "to follow

after," etc.; that is, he uses linguistic expressions which designate concretely the "state" or "condition" of an action rather than the abstract relations of time as such.

The concrete grounding of primitive man's language-forms in the givenness of sensory experience is evident also when we examine the various linguistic devices for the expression of numbers. In many languages, 'to count' literally means 'to finger,' and the terms designating specific numbers have a corresponding meaning. The linguistic means for the fixation of abstract number concepts have not yet been created.

Many languages reveal that the ideas of self and of selfhood were at first inseparable from the concrete conceptions of the body and its most important organs. Words designating "body," "intestine," "heart," "head," or "breath" serve as reflexive pronominal expressions, corresponding to our words 'myself,' 'thyself,' 'himself,' etc. Evidently the self referred to reflexively by the various body-part words was originally conceived in bodily concreteness, as a sensory-intuitive entity in space.

This fixation of the "self" in terms of the body is supplemented in some languages by the use of special space-designations. Locatives and demonstrative pronouns become the basis of that greatest device for grammatical substantialization, the article—a fact which emphasizes the dependence of "objectifications" upon space-conceptions and upon spatial integrations.

The sensory-intuitive basis of objectification is apparent also in the linguistic forms which make no clear distinction between verbal and nominal conceptions, between "action" and "being." The Eskimo, for example, does not say, "He stabs me," or "He sees me." He says instead, "My being stabbed by him is," "My being seen by him is." In such phrases the verbal idea has not yet become emancipated from the idea of things which may be owned. "Being" and "action" are still fused in the experiential manifold of "possession-action," of "being-action," and have not yet been conceived as abstract from each other. The verb is neither a real verb nor a pure noun, but a noun-verb—something between a real verb and a noun used as a verb.

Linguistic forms of this type lead to a strong personification of the verbal idea. In Chinook, for example, one says, "Once more her lie has done her," instead of, "She has lied again"; or, "His sickness came to be upon him," instead of, "He became sick."

If to this far-reaching personification is added the reduplication of

subject and object (and at times of the verb as well), we are confronted with linguistic expressions for which we have no equivalents in our thought-forms, and for which we lack the proper language-feeling.

Primitive man also uses concepts, but his concepts are not at all like ours. He forms them differently, and he uses them differently. We try to speak with logical precision; primitive man tries to depict. We classify, he individualizes. His concepts are predominantly image-concepts. His languages show an almost total absence of generic terms corresponding to general ideas, while they abound in terms designating specific persons, animals, plants, and things. The Tasmanians, to cite only one example, had no word for tree in general, but they did have words for every variety of gum tree or bush. Even more characteristically, they had no words to denote such qualities as round, short, tall, hot, cold, soft, hard, and so on. The meaning of 'round' they expressed by saying, "like a ball"; that of 'hard' by saying, "like a stone"; that of 'tall' by saying, "big legs"; and so forth. The linguistic expressions they accompanied by gestures in an attempt to re-create the object described before the eyes of the person spoken to. The concrete, sensuous-intuitive way in which primitive man comprehends the world about him could not be better revealed than through these language habits.

Intertwined with the sensory-intuitive content of words is the mystic conception of reality which is so characteristic of primitive thinking (cf. Boas; Lévy-Bruhl). For primitive man, objects and events are interrelated through a specific context of mutual "participation." There is no perception which is not somehow grounded in mystic relations, no sign which is not in some way more than a sign, no word which does not have its share of magic power. The use of words is therefore never a matter of indifference. The mere utterance of a word may produce beneficial or disastrous results. Knowing the name of a person gives one power over him. The person or thing is identified with his or its name, and the fate of one is the fate of the other. Extreme caution in the use of words is thus required. Special languages are reserved for certain occasions, and others for certain classes of persons, for men and for women. While hunting, the name of the hunted animal must not be uttered, and the names of gods are frequently taboo. During special ceremonies, words and songs may be used, the meaning of which is completely lost to those who hear them, and occasionally even to those who speak them; but this does not

detract from their effectiveness in the world of primitive man so long as the words are part of the tribal tradition, for he does not use these words to convey meaning in the logical sense. He uses them in order to achieve certain mystical ends, certain emotional effects.

As man's thinking advances and becomes more abstract, more conceptual in the logical sense, the descriptive forms and gestures are gradually eliminated and the mystic implications of words are suppressed. As soon as abstract concepts permit a freer and easier communication, the concreteness and graphic precision of earlier language habits, as well as the magic employment of words, are sacrificed. A rationalization or intellectualization of thought takes place, and gradually there emerge thought-forms which we may designate as modern; and with them come a new conception of language and its function, and a new use of words. Or at least the emphasis is shifted from communication to analysis—to the use of language in the search for truth.

The Growth of Language

The study of languages led Wilhelm von Humboldt to the formulation of a general theory of language according to which all natural languages were to be arranged in a single line of development from the most primitive form to the most advanced linguistic structure. Empirical research shows, however, that such a simple scheme is utterly inadequate for the classification of languages. Where Humboldt assumed the unfolding of an inner language form and postulated a corresponding uniformity of linguistic development, we find today the greatest imaginable variety of forms and functions, and the most divergent lines of progression. The idea of a universal language in Humboldt's sense is a fictitious abstraction. Only particular languages are real, and they are associated with specific speech-communities (cf. *A Philosophy of Science*).

Nevertheless, all languages have certain forms and structures in common. It would be strange were it otherwise; for, whenever a person uses a language, he must of necessity employ words or linguistic signs which designate that about which he talks, and he must use other words which signify what he says about it. That is to say, he must employ at least nouns and adjectives, and his use of these words must be governed by speech habits or "rules of grammar."

Some of the rules governing the use of words obviously reflect the distinctions and relations implied in experience itself. Noun-substan-

tives and pronouns are signs designating things; but, used in different contexts and with different emphases, they become subjects of action. The verb appears. Action frequently involves things acted upon, and the words designating these things stand in the objective case, thus giving rise to another grammatical category. Human relations, the bonds of kinship and of friendship, and the desire to possess provide the basis for the grammatical categories of relation. Reference to space and time—no matter how crudely both may be conceived—complete the list.

Unfortunately, the matter of grammatical categories is not quite so simple as this account of their origin seems to imply; for the various parts of speech do not always represent fixed categories but may fuse into one another. They most certainly are not identical with the categories of logic.

We have already seen that the human equation of personal and tribal interests and purposes is inescapably present in all linguistic forms and structures. The grammatical categories of classification, for example, rarely coincide with the class-concepts involved in our logically developed classificatory scheme; and grammatical gender, as we use it today, is essentially a remainder of primitive forms of classification and has little, if any, logical significance.

Other grammatical categories may correspond more closely to the logical categories, but the correspondence is never complete. Overlappings and intersections occur everywhere, and the grammatical categories are often mere foreshadowings of rational categories.

There are several reasons for this discrepancy of grammatical and logical categories. From the very first, grammatical categories reflect the practical attitudes and the bio-social activities of primitive man in his natural environment. They express his reactions to processes and relations in the sphere of things; and his reactions are permeated by a sense of mystic participation which is beyond our full comprehension and which certainly contradicts the laws of logic. Yet it is this mystic world-view of primitive man which furnishes the basis for the development of natural languages, and for the rules of grammar.

It is true, of course, that the growth of language has been largely the result of an intellectualization or rationalization of thought. But the morphological evolution of grammatical forms cannot be fully explained as a process of rationalization. New forms constantly come into use, but the old forms persist; and even the new forms are not always engendered by greater rationality. A desire for uniformity

tends to eliminate morphemes which have become unusual; but a desire to express new thoughts (or old thoughts in a new form) tends at the same time toward the creation of new morphemes.

Some of these changes may show uniformity of progression, development in specific directions—as does the development of modern high German (*Lautverschiebung*) out of older forms; or as do changes resulting from analogy, shortening, adequation, and so on, in all modern languages. Other changes are irregular and do not reveal any particular direction of growth. In either case it is true that the resulting changes arise from activities which have independent bearing upon the various phases of the grammatical system. It would be unreasonable, therefore, to assume that these non-logical factors which make for development in language should evolve a grammar which is logical in all its forms and rules.

Now, if the categories and rules of grammar have a pre-logical origin, and if they are modified through various non-logical factors—that is to say, if the categories and rules of grammar vary from language to language and are determined by the specific history of each language in question—then it is also evident that grammatical categories alone cannot fulfill all the requirements which logical thinking must demand of a language. The categories of grammar must be supplemented by the special categories of logic.

As a first step in the direction of such supplementation we shall now examine in greater detail the nature and function of language.

Language and Communication

Language, so we have seen, arises and exists only in speech-communities; and there it serves different functions. It may be employed for the purposes of magic; it may be put to emotive and hortatory uses; it may have a poetic or ceremonial function; and, lastly, it may be the vehicle for communication of ideas. In this latter function language plays its most important role and is of special interest to us. It is this function, incidentally, which also gives most of the other functions their communicative significance; for wherever words are employed they must have some denotative meaning, some reference to something other than themselves, or they are no longer *words* in the full sense of that term.

Language, however, the spoken or written word, is not the only means of communication. Gestures and pantomime and the "wink of the eye" also enable us to understand other persons. Communication

of some sort is found even in the animal world. Warning cries and mating calls are means of communication—although they are essentially cues for behavioral responses and nothing more.

Communication by language differs basically from communication by behavioral cues; for the linguistic expression is not merely a cue to behavior but also transmits an "understanding" of specific referents. That is to say, in linguistic communication words are used in such a way that the referents which the speaker has in mind will be brought to the attention of the hearer and will be determined for the hearer. Communication of ideas, in other words, presupposes the linguistic interpretation or analysis of experience.

How is communication of this kind possible?

Let us assume that a speaker, A, communicates linguistically with a hearer, B, using a spoken language only; and let us assume, furthermore, that A says, "This house is large." We stipulate that B has "understood" A if he can assert, deny, or question the truth of A's statement. We ask, What are the conditions which enable B to understand A?

The problem is twofold; for it is somewhat different for A and for B. For A, the speaker, it is a problem of finding words which express adequately the meaning he wants to convey. For B, the listener, it is a problem of interpreting those words so as to obtain the meaning intended by A. For A, the problem ends when his speech mechanism produces successfully the particular air vibrations which are the space-time instance of the words that were chosen to convey the intended meaning. For B, the problem begins when his ear has translated these air vibrations into the appropriate sounds. In the strict and literal sense, therefore, nothing is ever transferred from A to B when communication takes place; and still the gap between A and B is somehow bridged (cf. Britton, 1939; Urban, 1939).

This bridging of the gap would be impossible if A and B were particular and self-sufficient entities in the sense of Leibniz's "windowless" monads. Actually, the gap is bridged before communication begins, and communication itself is possible only because the gap is already bridged in some way.

After all, A and B, as human beings, are essentially alike in their basic modes of experience. They are alike not only in bodily features, not only in the functioning of their sense organs and in their urges and instincts; but they are alike also in their respective "mental structures." They are at least potentially capable of having similar experi-

ences. In other words, A and B understand each other because they are, in the same sense and to a similar degree, minds capable of comparable experiences (cf. Urban), and of comparable interpretations of their experiences.

Behind all communication, as its indispensable presupposition, lies a basic *kinship of minds* which transcends all merely social affinities —a "transcendental unity" of all minds capable of mutual communication. Where such kinship of minds does not exist, mutual understanding is impossible; and where it exists only to a limited degree— as between humans and animals—mutual understanding is fragmentary and restricted.

To be sure, A and B exist also in pretty much the same kind of a world as far as basic conditions are concerned, and, if speaker and hearer are members of the same speech-community, the "intuitive record of experience" is approximately the same for both. However, this similarity of their respective worlds for A and B is also dependent upon the prior affinity of their minds; for only minds which are fundamentally alike can live in the "same" world. This is so because the condition or status of the subject or "knower" determines in each case what is to be experienced and therefore what is to constitute its world.

It has often been pointed out that where there is no universe of discourse common to speaker and hearer, no communication can take place. Men may use the same words and still fail to understand one another because they use these words within different frames of reference, within different universes of discourse. The points of view of science, of poetry, and of religion constitute such different universes of discourse, and the manifold misunderstandings between persons representing them are too common to require illustration. If an understanding is to be achieved among men, then at least *some* "values" must be acknowledged by all concerned—although it is not necessary that all accept the same value *scale*. If the man of religion is to understand the scientist, he must admit that there is value in experimental procedure—even though he may regard revealed truths as more significant than the laws of science; and if the scientist is to understand the poet, he must acknowledge that there may be ways of looking at things which transcend the categories of science—even though in matters of science he insists upon experimental verification. An acknowledgment of common values, however, presupposes once more the mutuality of minds; for how could the same values be acknowledged if the minds thinking them were not akin one to the other and if they

did not interpret similar experiences in fundamentally the same way?

Assuming now the prior kinship of minds as an indispensable condition of communication, it would seem that communication presupposes also that each word used have one and only one meaning; that its referent be completely fixed and determinate. Such precision of language is indeed a desirable ideal, and one which modern science is trying to achieve; but it is not the only ideal which dominates the use of language.

In the next chapter we shall deal with this problem more thoroughly and shall view it in its relations to other aspects of the meaning-situation. It is sufficient at this time to have pointed out that in the use of language for purposes of communication the employment of language as means of interpreting and analyzing experience is presupposed.

CHAPTER II

SEMANTICS

Meaning as Mental Content

The problems of language and of the use of language so far discussed should be regarded as preliminary in our general analysis of language and meaning. We shall now turn to a consideration of the more specifically semantic problems and shall make an attempt to clarify at least the fundamentals of linguistic meanings.

Some salient facts stand out from our previous analyses. First and foremost among them is the recognition that meaning, and linguistic meaning in particular, is encountered only when some mind takes a sign or symbol as referring to or designating some referent. The sign or symbol may be a word. As a class of "similar" sounds (or "similar" marks), repeatable at different times and by different people, and designating different instances of a class of "similar" objects, a word must be distinguished from the mental content which it expresses as well as from the referent which it denotes. Its relation to the referent is intentional rather than causal; for only when a mind *intends* a sign to designate something does that sign have meaning.

It follows that a word (as a class of "similar" sounds or marks) never coincides with its meaning. That is to say, the meaning of a word is never identical with the sound or the mark which is an instance of the word. The meaning is in each and every case a reference to something other than the experiential complex which serves as the word. The word is only the means by which an understanding mind fixates an intended meaning. Without this mind-imparted reference to something beyond its own physical instance the word has no meaning and designates nothing.

In the preceding chapter we said that the self-transcending reference imparted to the words by a mind implies a referent. But does this mean that the meaning of a word is identical with its referent? The answer, I believe, must be an unqualified No.

In the first place, the word-transcending reference is beyond question a mental act, and without this act no word has meaning. Meaning, therefore, exists only for a mind, and only as a mind-intended complex,

as a "psychic entity." That is to say, meaning is not a *thing*, not a physical entity, not something that exists as a material object in a physically real world. If this is so, then, surely, meaning cannot coincide with the referent, if the latter is a material object. The *meaning* of the word 'chair,' for example, is not itself a physically real chair—although the *referent* of the word may be a real chair. If the meaning itself were identical with the referent we could never be in error about the latter. But error besets our thinking now and then and makes us painfully aware of the fact that an object "referred to" is not necessarily what we "meant."

It is relatively easy to see the difference between the meaning of a word and its referent when the referent is a material thing. It is not so easy to see it when the referent itself is something abstract—an ideal, a principle, or the like. But closer inspection, I believe, will reveal a difference even here. A principle, for example, may imply consequences which are not at once clear to us and which we do not "mean" or intend when we first formulate it. Similarly, the acceptance of a new ideal may entail a broad and far-reaching readjustment of our entire value-scale, although we did not mean all this when we first conceived the ideal. Furthermore, if meanings and referents were completely identical, discoveries would be impossible in mathematics; but discoveries have been made in mathematics. Moreover, if there is anything to the contention that 'democracy,' 'liberalism,' or other similar words, designate conditions or states of affairs possessing specific and discernible characteristics, then it must be admitted that the meaning of these words, being a mental complex, cannot be numerically identical with their referents; for the referents are actual (or ideal) states of affairs, but the meanings are our ideas of these states.

Finally, it is a fact that different referents can be referred to through the same meaning, and different meanings can refer to the same referent. For example, the phrase 'the book on my desk' may now have as its *referent* "*Webster's Collegiate Dictionary*," now "Kant's *Critique of Pure Reason*," now "Plato's *Republic*," depending on what particular book is lying on the desk when it is employed; but the *meaning* of the phrase remains substantially unchanged. On the other hand, the two phrases 'author of the *Critique of Pure Reason*' and 'the Sage of Koenigsberg' obviously do not *mean* the same thing, but both have one and the same *referent*, i.e., both refer to one and the

same person who, as person, is numerically identical with neither of our meanings.

The non-identity of referent and meaning may also be understood from the referential difference of statements such as: (a) the experiential object or referent denoted by the term 'this rose' possesses the experiential quality denoted by the term 'red'; (b) the meaning of the term 'rose' includes the connotation of the term 'plant.' It is the difference, in other words, between denotation and connotation, as these terms are understood in traditional logic.

Now, if meaning is not identical with the word used as its vehicle nor with the referent designated by means of the word, then meaning can exist only as mental content—as something intended or projected or, if you will, constructed by mind—a fact or condition *sui generis* of our mental life, which we must acknowledge without being able to account for it. Perhaps we might say that the meaning of a word is our interpretation or comprehension of the referent. It is then clearly marked as mental content.

However, not all mental content is necessarily meaning; and, surely, not all content is identical with the meaning of every word.

Elements in Meaning

All meaning involves a reference to something other than the sign or symbol itself. It involves an intended relation to a referent. No matter how vague or obscure the meaning of a word may be, the reference to "something beyond" is always present. Without it a symbol would not be a symbol, a word would not be a word.

Moreover, the actual nature of the referent provides the indispensable basis for the qualitative characteristics of meaning. That is to say, if the nature of a referent were other than it is, the meaning of the word which designates that referent would also be other than it is. If, for example, the actual rose were other than it is, the meaning of the word 'rose'—if the word were still to designate the same referent—would also be other than it is.

Nevertheless, the transcendent relation to a referent alone is insufficient to establish the meaning of a word. We have already seen that the referent is not identical with the meaning, and we must admit now that in different situations different aspects or attributes of the referent may be relevant to the intended meaning—which is another way of saying that the meaning of a word also depends on the context within which it is employed, and on our interpretation of the referent.

When we discussed the problem of the speaker in linguistic communication we found that it is he who selects the words to express the idea which he intends to convey; it is he who determines the manner in which to convey that idea. The word, therefore, is a vehicle for the meaning he has in mind. It follows that what a word means depends on who uses it and on the particular circumstances in which he uses it. It depends on the general situational context in which it is used. Hence, what the speaker thinks and feels about the referent is at least part of the setting which determines the meaning of a word and is therefore a constitutive element in the meaning.

This does not mean that words of a natural language do not have a conventional range of application; for they do. After all, the vocabulary of a natural language has been developed within a speech-community and has been formed largely through the linguistic interaction of the members of that community. If a word is to be serviceable at all as a means of communication, its applicability must be limited to some specified group of referents; and this limitation must be relatively fixed through conventional usage. The meaning of a word in actual use must fall within its established range, or communication breaks down and understanding is impossible. A linguistic utterance in a concrete speech-situation is therefore the product of the speaker's reaction to some specific referent plus the speaker's observance of certain linguistic conventions of vocabulary and syntax. The former, as experiential material, provides the basis for variation in the shadings of meaning; the latter serve as a norm for the choice and combination of words.

Taking it altogether, we may say that the meaning of a word—as encountered in actual communication—consists of those thoughts or ideas which the user of the word has of the referent to which the word refers. Subjective and objective elements are fused in establishing this meaning. To the extent to which this is true, a proper understanding of the nature of meaning therefore eliminates naïve realism as a tenable position.

Emotional Elements in Meaning

In the preceding chapter, reference was made to the fact that language was a means for expressing feelings and emotions long before it became an instrument for rational analysis. The rational or cognitive element has always been present in language, to be sure; but it was frequently subordinate to the emotive aspects. On the whole,

the development of natural languages has been toward increased rationality, toward a more rigorously precise cognitive use of words. Nevertheless, emotive elements persist in all speech. It would be strange if it were otherwise; for only that which in some way arouses man's interest finds linguistic expression; only that which does not leave him utterly indifferent becomes a topic for discussion.

To eliminate all emotive elements from the meaning of terms is unquestionably a desirable ideal of logic; and in a rigoristic language, such as mathematics or symbolic logic, this ideal may be realized to a remarkable degree. But our natural languages can never be developed in this manner. And if they could, it is doubtful that such a development would be desirable; for the purposes for which natural languages are employed are manifold, the logical communication of ideas being only one of the diversified uses. For all practical purposes it is sufficient to distinguish between linguistic expressions in which the emotive elements are subordinate to the cognitive function, and those in which the latter are subservient to the former. As long as we understand that the emotional elements contribute nothing to the cognitive function, and may even interfere with it, we can guard against them whenever pure cognition or rational communication is our goal. And if need be, we can construct a special language for this specific purpose, as we do in the sciences.

The emotional elements involved in meaning derive from the subjective attitude we take toward a referent, toward our listener, or toward both. They derive, in other words, from the general situational context within which we resort to linguistic expression.

Since our attitudes may vary in uncountable ways and may be simple or complex, the number and variety of emotional elements attached to, or involved in, linguistic meanings is legion. We may assert, question, doubt; we may hope, desire, reject; or we may sympathize, love, or hate. Something may fill us with pride, contempt, or disgust; it may give us pleasure or pain or fill us with confidence; it may call forth derision, irony, or sarcasm—and so forth throughout the whole gamut of the emotions. And every shade of our emotional experience may color the meaning of our linguistic expressions in actual speech.

But how do words express emotions? In the first place, the emotion itself may be the referent of the word: "I am very glad to be of assistance to you"; "I feel nothing but contempt for the scoundrel." In these statements the words 'very glad' and 'nothing but contempt'

refer directly to the emotions involved. They provide linguistic recognition for the emotions in an essentially cognitive manner.

The word 'scoundrel' in the second example, however, expresses also an emotion; but it does so in an entirely different manner. Here the emotion appears as a specific emotive coloring of a cognitive meaning. It is not itself the referent, but a mode of viewing the referent. That is to say, it is primarily a coloring due to subjective context. And it is this particular type of emotive element in meaning which so frequently interferes with a complete cognitive understanding of a linguistic expression. Words of endearment and abuse, the so-called "purr" words and "snarl" words, illustrate the type; but the range of these words is wide and their employment almost unlimited.

The emotive import of the words in question is actually part of their lexical meaning, part of their semantic range within a speech-community. It may arise (a) because the referents in question possess persistent emotional significance for the speech-community—in which cases the words referring to them reveal permanent emotional coloring; or (b) because situational contexts "point up" the referents and make them emotionally significant—in which case the emotional coloring of the words is incidental to their meaning. 'Magnificent,' 'dramatic,' 'voluptuous,' 'disgusting,' and so on, are words belonging to the first group. 'Freedom,' 'mother,' 'baby,' etc., are words belonging to the second group. The word 'mother,' for example, may have a purely cognitive and unemotional meaning—as when we say that in "mother-right" cultures inheritance and tribal relations are traced through a person's mother; or it may be emotionally "loaded" —as when we think affectionately of our own mother.

The explanation of the emotional coloring of meanings must in either case be sought in the psychological context or setting in which the words are employed. If the referent itself arouses an emotion within us, it seems only natural that this emotion should become attached to the meaning of the word denoting that referent. But even when our emotion is aroused by the speaker (or by our listener) it is understandable how that emotion can become attached to the meaning of the words employed; for it is a well-known fact that an experienced emotion permeates our whole being, that it determines in specific ways all our responses to our surroundings, and that it conditions our attitudes. Hence, when our emotions are aroused, every word heard or spoken by us is partly conditioned in its meaning by the emotional

context within which it is experienced. If, for example, we have become suspicious of the intentions of the speaker, we are inclined to find hidden meanings in what he says, and our suspicion of his intentions is carried over into a suspicion of his meanings—and this despite the fact that our suspicion may not be justified objectively. When we are disgusted with someone or something, our disgust colors the language we use, or it betrays itself in our manner of speaking—adding emotional significance, a pointed emphasis, to the words we employ. If we write rather than speak, various linguistic devices—such as figures of speech, exaggerations, understatements, change of word-order, etc.—may be employed for the same purpose; and from the linguistic context as a whole, individual words may attain an emotional significance which they do not ordinarily possess—as is the case in poetry and in some prose compositions.

Context and Meaning

Let us now disregard for the present the emotional significance of words and let us concentrate on their cognitive meanings. We then discover that in ordinary speech isolated words and sentences do not occur; for the unit of speech is a sentence or, at the least, a sentence-word, i.e., a word which functions as a sentence; and each sentence is an element in a larger context of communication, or is interwoven with the concrete experiential situation. Isolated words exist only as lexical components of a language, and sentences as well as words, *as isolated*, are used only in language studies. It is therefore appropriate to distinguish between the *actual* meaning of words and sentences as they are employed in speech, and their *lexical* meaning which we find in dictionaries and in books on grammar. For example, if the sentence, "The automobile was parked on the wrong side of the street," is part of a coherent communication, the general situational context and its relation to other sentences determine the particular referent and the actual situation referred to. The meaning of the sentence is concrete and specific. But when we consider the same sentence in isolation, the case is quite different. We know of course what an automobile is; and we know also, in a way, what is meant by 'being parked on the wrong side of the street'—although an Englishman and an American may differ as to which side of the street is the "wrong" side. We do not know, however, what particular automobile is referred to, nor where, when, or in what specific sense it was parked on the wrong side of which particular street. In short, the meaning of the isolated sen-

tence does not determine a definite "place" for any particular automobile in any actual or imaginable universe. We are at liberty to supply any number of possible referents for the sentence, and each will be equally acceptable as long as it is in accord with the lexical meanings of the words. This is so because the sentence is now no expression of a speaker's actual intention; it has no relation to a contextual situation which alone can give it concreteness and specificality.

As distinguished from actual meaning, the lexical meaning of words, therefore, indicates only a general "sphere" of the referent, the direction, so to speak, in which we may look for it; and it determines the *general range of applicability*. Only the context of the actual speech-situation determines the concreteness and specificality of meaning (cf. Cunningham, 1938, 1943).

When we analyze the context which gives concrete and specific meaning to linguistic expressions, a further distinction seems necessary. For example, the statement, "I hope it works,"—the meaning of which is completely determinate and specific in the usual speech-situation in which it would be made—is almost completely devoid of meaning when considered in isolation. The words 'I,' 'it,' and 'works' may denote so many different referents in so many different combinations that the lexical range of the phrase approaches infinity and a corresponding indefiniteness. The *situational context* alone can eliminate the vagueness inherent in the linguistic expression as such; and any shift in the actual context—i.e., any shift in speaker or object spoken about—produces a corresponding shift in the meaning of the statement.

It is possible, however, to fixate the meaning of such statements by presenting the experiential context in the form of a supplementary *linguistic* context. Linguistic context, in other words, may take the place of situational context (cf. section on levels of cognitive meaning).

Vagueness and Ambiguity

The emphasis we have placed upon context as a determinant of meaning should not mislead us into believing that words have no meaning at all outside a specific context. Context determines meaning, to be sure; but it does not determine all there is in meaning. Each word has its own more or less permanent *lexical meaning*. It refers to, or designates, certain referents rather than others. It has a limited *range of applicability*. Were it otherwise, the selection of words for the purpose of communicating ideas would be impossible and, in conse-

quence, linguistic communication could not take place. It is because words have a lexical meaning that a speaker can choose them as a vehicle for his thoughts and that a hearer can understand what is meant. Moreover, the lexical meaning of words is an indispensable prerequisite for the interpretation and analysis of experience.

In order to demonstrate the lexical meaning of words we need only change a word in some meaningful sentence and see what happens. For instance, if we replace the word 'red' by the word 'blue' in the sentence, "Some roses are red," the statement is obviously no longer true; its meaning has undergone a radical change and is now no longer an adequate interpretation of actual experience.

The meaning of words, however, is at times vague and imprecise—and not only so far as concrete denotation is concerned. All words which designate an infinite or an unstable denotation are, of course, denotatively indefinite. But there are other words—words which are connotatively indefinite; and their vagueness may be one of three possible types. (1) It may be an intentional vagueness—as it is in the case of meanings such as 'about a dozen,' 'more or less,' 'many,' 'roundish,' 'yellowish,' 'thirtyish,' etc.; (2) it may be a vagueness resulting from the comparative significance of the meaning itself—as it is in the case of such words as 'young,' 'old,' 'rich,' 'poor,' 'great,' 'small,' etc.; or (3) it may be a vagueness resulting from the impossibility of delimitating with precision the intended referents—as it is the case with words such as 'chin,' 'hip,' 'thigh,' 'red,' 'orange,' and so forth.

Of these three types of vagueness, the first, being intentional, constitutes no linguistic problem. At the most we can demand of the speaker that he be more definite. The second type, arising from an indefiniteness of the intended frame of reference, can be eliminated by providing the proper setting for the required comparison. A mouse may be *large* in comparison with flies, mosquitoes, and tubercle bacilli; but it is *not* large in comparison with dogs, horses, or elephants.

References to "here" and "now" involve a related ambiguity; for 'here,' for example, may mean "here in the United States," "here in Nebraska," "here in this house," "here on this page," "here at this point," or any number of similar "locations in space." Its precise meaning must be determined by context.

The third type of vagueness cannot be eliminated from language. It is inherent in the nature of the referent. Even the most concrete context of an actual speech-situation does not eliminate all ambiguity;

for there is no clear-cut division between *chin* and *cheek,* between *red* and *orange*—one referent fuses into the other. Fortunately, vagueness of this type is not too common in modern languages, and it is not of such radical nature as to render communication impossible. It is, of course, more dangerous when we deal with abstract notions, with values and ideals, than when our discourse is restricted to the material world. The only remedy is further analysis of the intended referent and a more precise specification of its boundaries.

Levels of Cognitive Meaning

In the two preceding sections we have pointed out the significance of context as a determinant of meaning; but we have shown also that lexical meaning is, in a measure, independent of context. Upon inspection we thus find that at least three distinct levels of cognitive meaning may be distinguished.

The first and lowest level is that of *lexical meaning.* At this level we find the vocabulary of a language, the *isolated words* with their defined range of applicability. Here meaning is determined exclusively by rules and procedures of definition.

That this actually is a level of meaning becomes clear when we compare the experiential significance of isolated words—such as 'house,' 'freedom,' 'zero'—with that of meaningless syllables—such as 'phroof,' 'gaablus,' 'sleethitz.' The former clearly point in specific directions of applicability; the latter leave us bewildered and without the slightest orientation toward a referent.

Isolated words, however, play no part in actual speech; for they do not constitute a unit of communicable meaning. Words must at least be used as sentence-words, or must be combined in sentences; and the meaning derived from this combination must augment and determine further their lexical meaning. The first level of meaning, therefore, must be supplemented by a second. The level of lexical meaning must be supplemented by the level of *syntactical meaning.*

At this new level, meaning is fixed and determined by the rules of syntax; for not every combination of words is meaningful. "Red true of which," far from having a more determinate meaning than that possessed by the individual words which enter into the combination, actually makes less sense to us than would each word taken by itself. The conjoining of these words in such an "ungrammatical" manner interferes even with their lexical meaning.

However, not every sentence which is constructed in strict con-

formity with the (grammatical) rules of syntax is meaningful. Observance of the rules of grammar is necessary but not in itself sufficient. Our discussions have shown that complete concreteness and specificality of meaning can be attained only within the concrete context of an actual speech-situation. This context, therefore, determines a third level of meaning—a level which supplements lexical and syntactical meaning and which we shall call *contextual meaning.*

If the actual or experiential situation which completely determines the meaning of a sentence cannot itself be produced, it may be replaced by a "situational description"—i.e., it may be replaced by a group of sentences joined together under the dominance of our conception of the total situation involved. The sentences in question, in other words, must then be combined in such a way as to form a contextual whole which, as an integrated whole, has unitary meaning. Such a "situational context," be it actual or linguistic, is the minimum prerequisite for complete understanding.

It must be noted, however, that a situational context may be interpreted linguistically in various ways—the interpretations ranging from "pure description" in terms of conjunctions to "causal explanation" given in terms of implicative relations; from "enumerated contingencies" to "systemic interdependencies." It is at the contextual level, therefore, that we encounter *knowledge* in the full sense of that term, and, in particular, knowledge as science and as philosophy.

It has been suggested that syntactical meaning must be supplemented by logical meaning also; for, so the argument runs, "Virtue is blue," "Squares run courageously" are statements which in every respect conform to the syntactical rules of the English language and which yet are meaningless because they combine in an illogical manner words having lexical meanings in different universes of discourse. I myself have argued in this manner in a previous publication.[1] But I added at the time that "the grammatical categories alone are insufficient *for a concise and objectively valid* (i.e., *true*) *expression of meaning.*" As long as we insist upon this qualification, the argument has a semblance of force. But meaningful statements need not be true. Not even the positivists would maintain that falsity makes a statement meaningless.

The statements "Virtue is blue" and "Squares run courageously," if taken literally, I now regard as false rather than meaningless; and

[1] Cf. "The Meaning of 'Meaning' Re-Examined," *Philosophical Review*, XLVII, 1938, 260-261.

they are false because analysis reveals an incompatibility of the lexical meanings which are conjoined in the respective sentences. A recourse to logic is had only in so far as all meanings are at all times subject to the basic laws of thought—to the law of identity, of contradiction, and of the excluded middle. There is no need for a special level of "logical meaning" as distinguished from the three levels already discussed; for the laws of thought permeate all levels.

Metaphors

I am fully aware of the fact that it has become customary to regard metaphors as meaningless and to eliminate them from further consideration in linguistic analysis. I am not so sure that this completely negative attitude is justified. A phenomenon so common in natural languages as are metaphors may be expected to be of some significance in linguistic communication; why else should it have been developed? Reference to the merely emotional value of metaphors is, in my opinion, not sufficient to account for their widespread use. Metaphors are valuable not only in poetry but in broader aspects of culture as well. It was largely through the metaphorical understanding of words that language was first liberated from its sensory-intuitive basis in primitive experience; and it was again largely through the metaphorical use of words that man first learned to project his ideals beyond the actualities of space-time experience. His greatest hopes and aspirations as well as his greatest fears he voices even now predominantly in metaphors. If metaphors were actually meaningless, then vast areas and some of the most important phases of human culture would remain forever unintelligible and impenetrable.

Etymologically, the word 'metaphor' (derived from Greek *metapherein—meta* beyond, over; and *pherein* to bring, bear) means *to carry over, to transfer*. More specifically it has come to mean "the use of a word or phrase literally denoting one kind of object or idea in place of another by way of suggesting a likeness or analogy between them." This definition, however, is still a matter of controversy and may require modification.

We shall here distinguish between a *non-figurative* transfer of meaning from one universe of discourse to another—to be called *transfer*; and a figurative transfer or the use of *metaphor proper*. Admittedly the distinction is not absolute and may not be acknowledged by some. But, as here understood, *transfer* serves a predominantly

cognitive function, whereas *metaphor proper* is primarily a device for emotional expression.

Transfer, as a rule, is based upon an obvious similarity of the referents. It is encountered most frequently in science and technology, where the appearance, the function, or the position of intended referents discloses a basic similarity. We thus speak of the *foot* of a mountain, the *mouth* of a river, the *eye* of a needle, the *shoulders* of a road, the *wings* of a collar, the *keys* of a piano, the *legs* of a table, the *arms* of a chair, the *neck* of a bottle, the *teeth* of a gear, the *head* of a nail, the *bleeding* of tanks, the *migration* of ions, the *walls* of a cell, the *root* of a hair, the *labyrinth* of an ear, etc.; and terms originating in the physical sciences—such as 'momentum,' 'inertia,' and 'drag'—may be transferred to the realm of the social sciences, there to be applied to social phenomena and to social 'forces.'

Under the heading *transfer* belong also some usages of words like 'in,' 'under,' 'take,' 'give,' and so on. Thus, when we say that we have a problem *in* mind or a proposition *under* consideration, or that we *take* heed or *give* advice, we use the italicized words with a transfer of meaning; we use them metaphorically—in the *broad* sense of the term.

However, metaphors in the *narrower* sense, or *metaphors proper,* not only involve such a transfer of meaning, they also are strongly colored with emotional overtones and are suggestive of emotional responses.

A number of words may occupy a position somewhere between the two main groups, showing that the division is not absolute. We may, for example, speak of *sharp* tones, or *flat* tastes, or of a *brilliant* style; and the italicized words, unquestionably used here with a transfer of meaning, may have as much emotional significance as they have cognitive meaning. But when we insist that stocks are *watered,* markets *flooded,* prices *slashed*; that corporations *milk* the public, that politicians are engaged in *log-rolling,* that they are *slicing the melon,* that the taxpayers are *holding the bag,* or that we have money *to burn,* the emotional overtones begin to outweigh the cognitive meanings of the words used.

The real significance of metaphors is disclosed in their employment in poetry and in the poetic use of language: "The self-same beat of Time's wide wings"; "Can death be sleep, when life is but a dream?"; "Time, that aged nurse, rock'd me to patience"; "Thou still unravish'd bride of quietness, Thou foster-child of silence and slow time"; "rain-

drops typewriting on the roof"; "the fingers of wind brushed my face"; "darkness crumbles away"; "the hot and sorrowful sweetness of the dust"; "the eloquence of a silent kiss"; "wind writing its saga in the dunes of sand"; etc., etc. The uses of metaphor are legion!

What interests us here, however, is not simply the fact that metaphors are employed in a speech-community, but what purposes they serve and how their use can be justified (cf. Stern, 1931).

To begin with, the frequency of metaphors in everyday life and in literature (poetry and prose) suggests that they provide a convenient way of saying something that cannot otherwise be said so effectively.

As in the employment of all words, it is the speaker who, in the last analysis, determines the meaning of metaphors. It is he who sees the possibility of a transfer of meaning from one universe of discourse to another, and of utilizing a metaphor to express the ideas he has in mind. His comprehension of relationships, therefore, provides at least part of the basis upon which the meaning of metaphors rests. The hearer must adapt himself to the intentions of the speaker and must try to comprehend the speaker's meaning. In these respects the employment of metaphors does not differ from the employment of any other linguistic device.

This means, however, that a metaphor taken in complete isolation has no more (if as much) meaning as an isolated word or phrase has. Here, as in all other cases, it is the context—the linguistic as well as the situational context—which really determines the meaning. It is the configuration of concomitant circumstances which alone makes a metaphor intelligible.

Most, if not all, metaphors depend on a *tertium comparationis* for their transfer of meaning, upon a similarity between the primary meaning of the words used and their secondary or transferred meaning. This similarity does not find explicit expression in a metaphor (as it does in a simile), but is always implied or suggested. It must at least be present to the mind which comprehends the metaphor. It must be clear, striking, and adequate; if it is subtle or elusive, a certain amount of "preparation" may be necessary before the metaphor is understood. Knowledge of the subject-matter, comparable experiences, a general "affinity" between speaker and hearer, and a mutual sharing of opinions on the subject referred to will aid in the comprehension.

Now, the element of similarity, that which primary referent and secondary referent have in common in the experience of the speaker, may be some quality of structure or function, or it may be a "feeling

tone," i.e., it may be the way in which they affect him emotionally; and usually it is both of these factors intertwined. When a Christian speaks of God as *Father*, the metaphorical use of the word 'father' expresses not only an emotional reaction on his part but a far-reaching characteristic of the meaning of the word 'god.' Arguments from analogy, in the strict sense in which Thomas Aquinas intended them, would hardly be meaningful without recourse to metaphors or to metaphorical extensions of the use of words.

In each metaphor there must be at least as much cognitive meaning as is necessary for the identification of the secondary referent. Without this minimum of meaning no idea whatever could be communicated by means of metaphors. But once the referent of the metaphor has been identified, the purely cognitive meaning recedes into the background and gives way to the emotional significance which the speaker intends to convey. The metaphor becomes an emotive expression and, as such, gains its poetic status. It expresses the speaker's feelings and is aimed at evoking corresponding feelings in the hearer.

To infer from this fact that all metaphors have only an emotive significance would, however, be somewhat rash; for metaphors place their actual referent in a new context, thereby revealing new attributes or disclosing old attributes in a new light, thus adding to our comprehension—at least to its vividness and completeness.

The experiential basis which makes possible the meaningful employment of metaphors must be seen in the fact that few, if any, referents have only one characteristic. A linguistic expression denoting a given referent includes, therefore, a plurality of constitutive elements in its meaning, and any one of these elements may be in the focus of our attention when we use the expression denoting the whole. The actual context of the speech-situation determines in each case upon what aspect of the referent our attention is centered and what element of meaning is central at that time. If this element is sufficiently prominent in our attention, i.e., if it has become the central meaning for the given context, it may lead to the recalling of some word or phrase which denotes it more directly (although again a whole complex of associated elements may cluster around it); and this word or phrase now becomes our metaphor. Consider for example the following metaphor: A ship *plows* the sea. In its forward motion the ship's prow heaves up the water in a manner strikingly similar to the way in which a plow throws up the ground. But only swift forward motion of the ship produces the effect; and, hence, the special "overtones"

of the metaphor. In the metaphor a carry-over of meaning from the sphere of the primary referent (what the *plow* does) is fused with the actual meaning as determined by the experiential context (what the *ship* does). The fusion of elements of the primary meaning with the actual meaning as determined by the experiential context *is* the metaphor. And because it is such a fusion of meanings, a metaphor can never be fully translated into non-metaphorical language. Something of its quality, of its overtones, of its meaning is lost in the translation.

It is obvious that not all metaphors are of equal value; for not all contribute equally to a fuller and more vivid comprehension of a referent. A good metaphor must be clear and to the point, and must be effortless. While a *post factum* transcription of the intended meaning may justify the use of some specific metaphor, a metaphor is useless if it does not authenticate itself within the speech-situation in which it is employed. In addition, a good metaphor must present the actual referent in a new and revealing relation and thus must add to our experience and to our comprehension.

The success of metaphors as a means of communication depends upon our ability to discern the unique qualities of a thing, person, or situation, and to see at the same time what qualities this thing, person, or situation has in common with other, more concretely familiar, objects of experience. Only if we take metaphors literally are we in danger of confusion of thought; but few adults ever take them so.

The contention that metaphors are "meaningless ornaments of discourse" and a threat to clear understanding is only conditionally justified. If we assume that linguistic meaning is completely determined by lexical and syntactical rules, then metaphors are indeed meaningless; for no rules of grammar alone ever warrant the transfer of meaning from one sphere of referents to another. But linguistic meaning—at least the meaning of natural languages—is never completely determined by lexical and syntactical considerations alone. The whole concrete context of an actual speech-situation is always involved. And this context makes metaphors meaningful and effective.

Admittedly, communication of ideas is one thing, and proof and verification of ideas is something else; and for proof and verification metaphors may be inadequate—unless, of course, they can be reduced to a simple transfer of meaning or can be adequately translated upon demand. If this is the case, metaphors are nothing but "shorthand" expressions for complex descriptive statements concerning which full agreement may or may not be attainable. The difficulties which we

encounter when we try to transcribe the full meaning of a metaphor do not force us to conclude that metaphors have no meaning. Only a narrow conception of language and of the use of language can force us to such a conclusion. Natural languages, employed in concrete speech-situations, will always abound in metaphors because metaphors facilitate *intimate* communication and enable us to project ideas beyond the sensory-intuitive basis of understanding.

Empirical Context and the Pragmatic Significance of Meaning

The actual context of the speech-situation is one of the major factors determining the specific meaning of our words and sentences. It provides a supplement to lexical and syntactical meanings which is indispensable in the concrete employment of natural languages. There is, however, another aspect to this concrete situational context; for language is employed not merely for the purpose of transmitting information but also, and often primarily, for the purpose of modifying the hearer's attitudes or behavior. It is employed, in other words, to induce a hearer to accept a certain belief, or/and it is employed to stimulate the hearer to action. What linguistic expressions *mean* is therefore supplemented by what they are *intended to accomplish*. Their meaning is augmented by their *pragmatic significance* (cf. Morris, 1937, 1938).

If we try to lead some person into accepting a certain belief, we must impress upon him the merits of this belief. We must persuade him in some manner to accept our statements as true. However, if the hearer is not completely without experience, he will know that not all statements are true, and he will want to know the reason why he should accept the new belief. He will want to examine and evaluate the evidence. Actually he may take one of three possible courses of action: (1) He may refuse to believe what we say, or refuse to accept it as true, unless or until the evidence available convinces him of its truth. (2) He may accept it tentatively or as an "hypothesis," i.e., he may refuse to reject it as false, unless or until the evidence available convinces him of its falsity. Or (3) he may refuse to consider the statement, or refuse to regard it as "meaningful," so long as it is inconceivable that there ever will be any evidence forthcoming which will tend either to verify or to disprove it (cf. Stace, 1944).

If such are the possible courses of action for the hearer, then the speaker, if he hopes to persuade his hearer to accept a certain belief, has only two choices of action: either (1) he must offer convincing

evidence and thus settle the issue there and then; or (2) he must persuade his hearer to accept the new belief as an hypothesis to be confirmed (or disproved) by evidence still forthcoming.

Unless we deal with analytic propositions, the evidence presented must be empirical. If, however, it were manifest from the beginning that relevant empirical evidence (either for or against the belief) could not possibly be forthcoming, then the hearer would rightly refuse to entertain the belief even as an hypothesis and would reject it as *meaningless for all practical purposes.*

The Verifiability Theory of Meaning

The "practical meaninglessness" pointed out in the preceding section seems to provide the experiential basis for the so-called verifiability theory of meaning developed by logical positivists and logical empiricists.

In its original form (Schlick, Blumberg, Feigl) this theory asserted that "a proposition has meaning only in so far as it can be verified"; that whenever verification is impossible, "the proposition is meaningless." In other words, meaning *is* verifiability and verifiability *is* meaning.

A refutation of this original theory of the logical positivists is rather easy (Ducasse; D. L. Miller; H. Miller; Sidgwick; Stace; Weinberg; Werkmeister; Wiener; Wisdom). It suffices for this purpose to apply the principle of verifiability to the theory itself. How, in other words, can the statement that only verifiable statements are meaningful be verified? How can we verify the statement that verifiability is meaning and meaning is verifiability?

To begin with, the criterion of verifiability is not a tautology. If it were, its denial would yield a formal contradiction; but no such contradiction can be discovered. Furthermore, since the theory gives information which cannot be proved through analysis alone, it is manifestly synthetic and requires non-analytic proof. But such proof has not only not been presented, it is logically inconceivable and will therefore never be forthcoming.

The theory was developed for the specific purpose of eliminating all metaphysical propositions from the realm of philosophical discourse. Its authors would therefore disdain metaphysical arguments in support of their position—even if such arguments were conceivable. But no metaphysical arguments can justify the theory; for if the theory is true, all metaphysical statements are meaningless and mean-

ingless statements cannot prove a theory true. Whatever evidence there is to support the theory of verifiability must therefore be of an empirical nature.

But the assertion that only verifiable statements are meaningful is a universal proposition, and universals cannot be empirically verified. At best they may be *confirmed* by empirical evidence, i.e., they may find *some* support in empirical facts. If we grant, for the sake of argument (subject to revision), that certain meaningful statements are indeed directly verifiable—all statements, namely, which pertain to the immediately experienced quale of sense-data—then meaning and verifiability fuse here into a unitary experience. The meaning of 'this is red,' for example, is immediately verified when *this*, i.e., the particular quale referred to, is indeed *red*. However, not all statements are statements concerning sense-data; and to reason that, since meaning and verification fuse experientially for one class of propositions, they must be *identical*, and identical for *all* types of propositions, is, to say the least, a very hazardous venture.

Nor do we find ourselves on safer ground with the contention that all meaning is ultimately exhaustible in terms of sense-experience, and that there is no meaning beyond a direct or implied reference to sense experience. A sense-data theory of meaning would, in turn, fall short of providing its own justification; for the meaning of the contention that everything meaningful is reducible to sense-data is not itself so reducible.

But if the verifiability theory of meaning is neither analytically nor metaphysically nor empirically verifiable, then it is not verifiable at all. And if it is not verifiable, then, by its own criterion, it is meaningless. The verifiability theory of meaning, therefore, cannot be both true and meaningful. It is either false or meaningless; and I believe it can be shown to be false (cf. Stace).

If the verifiability theory cannot be proved true, its advocates may assert that this does not diminish its value since it is a postulate, a stipulation, or convention, which requires no proof and which must be accepted for the sake of the system that is to be created by its aid. If this is their contention, then, of course, there is no reason why anyone should accept such a postulate or convention. There is, on the other hand, good reason for rejecting it; for the verifiability theory sets arbitrary limits to the range of linguistic expressions and precludes as meaningless certain statements concerning human experience which are eminently not meaningless—such statements, namely,

as pertain to the mental life of "other" persons or to events of the past, or statements which are universal in scope.

The Modified Theory

In view of the difficulties encountered by the original version of the verifiability theory of meaning, its advocates "toned down" their "criterion" to the assertion that a proposition is meaningless unless there is some experience or some element of experience which has some bearing upon it, which "tells or might tell for or against it," or which "tends to confirm it or tends to refute it" (Ayer).

In this new form, the theory becomes vague and ambiguous; for it is possible to find "relevant" evidence in connection with almost any proposition. Metaphysical propositions, for example, may be relevantly connected with certain elements of experience. The observed order in the universe and the apparent purpose in evolution and history can hardly be regarded as irrelevant to a theory of ultimate reality. Order and purpose are factors which tend to confirm one metaphysical doctrine rather than another. They tend to confirm a theistic view of the world and tend to disprove mechanistic materialism (D. L. Miller; Stace, 1944; Wisdom).

What is true of metaphysical propositions is true also of empirical propositions. On the one hand, it is extremely difficult to find an empirical proposition with respect to which no relevant experience could possibly be conceived. On the other hand, it may be impossible to determine what relation an observed fact has upon a given proposition. If a physician asserts that his medicine will cure a certain patient and if, after taking the medicine, the patient does recover, what bearing, if any, does his recovery have upon the physician's assertion? Or, to use another example, to what extent does the victory of the United Nations tend to confirm the proposition, "Right will always win"?

A further difficulty arises from the fact that the verifiability theory of meaning is concerned exclusively with the meaning of propositions. It is a fact, however, that questions, commands, and exclamations also have meaning. Were it not so, we could never reply to a question or carry out the command. Questions, commands, and exclamations can be neither confirmed nor confuted by experience. The whole idea of verifiability is here irrelevant. The verifiability theory of meaning is therefore at best incomplete. It is too narrow in its range of appli-

cation. But difficulties arise even if the theory is considered in its narrow sense.

In making any meaningful statement, at least two logically discernible steps are involved: (1) Words must be chosen to express the intended idea, i.e., they must be chosen because of their lexical meaning; and (2) these words must be combined in conformity with the rules of syntax, i.e., the statement as a whole must be given syntactical meaning.

Now, positivism or logical empiricism seems to be concerned only with the meaning of the whole statement. The criterion of "confirmability" is hardly applicable to individual words; for the meaning of words can be neither "confirmed" nor "confuted." It can only be defined. The rules of syntax, moreover, are not derivable from the criterion of confirmability, nor can they be established in some other way by means of that criterion. It follows, therefore, that the theory under investigation can have no bearing upon lexical and syntactical meaning and that, as a consequence, it does not affect one way or another the two basic levels of linguistic meaning.

If it now be maintained that actually a definition of the lexical meaning of words is intended, we are thrown back upon the sort of empiricism advocated by David Hume. And if, for the sake of argument, we admit that only such words are to be regarded as possessing meaning as refer to objects which are either directly observable or are reducible to observables, it can be shown that the theory contradicts its own basic contention—the contention, namely, that only confirmable (or confutable) statements have meaning.

Consider, for example, the words 'object,' 'quality,' 'sense-data,' 'physical,' 'exist,' 'observable,' 'reducible,' 'not,' and a few others of a similar type. None of these words are meaningless or are said to be meaningless by the positivists. Let us assume even that their meanings are of an "empirical origin," i.e., that they can be traced back to certain sense-impressions—either as suggested by Hume, or in some other "empirical" manner.

Now, these words, which possess an "empirically determinable" meaning, can be combined in strict conformity with the principles and rules of grammar into various sentences, such as "Physical objects exist," "Physical objects possess observable qualities," "Sense-data are not qualities of physical objects," "Physical objects are not reducible to sense-data," and so on. All of these sentences are regarded as meaningful by the positivists, and as meaningful in the sense of the

criterion of confirmability. But the same words may also be combined in strict conformity with the rules of grammar into a sentence such as this: "There exists a physical object which can never be observed and whose qualities are in no way related to our sense-data"; and this sentence is "meaningless" as judged by the criterion of confirmability, for no evidence of the sense-data type can ever be relevant to it.

It may be granted that for all practical purposes the statement is of little importance; but does this mean that it is actually meaningless? We cannot possibly know what, if any, evidence is relevant to any statement without having first understood the statement; and how can we understand it unless it already has meaning? The choice of "operations" which may or may not tend to verify it, presupposes that we understand what the proposition means. How else could we choose the "operations"? Or, if the proposition is metaphysical, how else could we know that it *is* metaphysical and therefore beyond empirical confirmation or refutation? Confirmability (and refutability) is in all cases subsequent to meaning and therefore is not a constituent element of meaning—at least not of all meaning.

To be quite specific, confirmability is a constituent element in neither lexical nor syntactical meaning, nor in the referential meaning determined by the concrete context of a speech-situation. It *is* a constituent element in the *practical* meaning discussed previously. But such "practical meaning" is not meaning in the sense of designating or denoting referents; it is not meaning in any cognitive sense and might better be called *significance* or *importance*. The verifiability theory of meaning, even in its modified form, is therefore no answer to the semantic problem, i.e., to the problem of meaning proper.

Pure Semantics

In his *Foundations of the Theory of Signs,* C. W. Morris has distinguished between three aspects of the "science of language" which, up to now, we have not clearly separated. If, in an investigation of language, explicit reference is made to the user of a language, to the reasons for his employment of linguistic signs, to the ends to be attained, then, according to Morris, the investigation belongs to the field of *pragmatics.* If reference to the user of the language is omitted and if only words and their relation to referents or to designata are considered, we are in the field of *semantics.* Finally, if the relation to referents is also neglected and if the study is restricted to words

and their syntactical relations, then the investigation belongs to the field of *syntax*. In the discussions which follow, we shall neglect the field of pragmatics and shall restrict our inquiries to problems of semantics and of syntax.

It will be advantageous, however, to accept a further distinction, one made by Carnap—the distinction, namely, between "pure" and "descriptive" semantics, and "pure" and "descriptive" syntax; for we shall have occasion to deal with both.

'Descriptive semantics,' according to Carnap, is the empirical investigation of the semantical aspects of natural languages; while 'pure semantics' is the analysis of systems of semantical rules. The division of syntax is analogous. In both cases the relation between the "pure" and the "descriptive" fields is comparable to that between pure and applied mathematics, or between mathematical and physical geometry.

It is Carnap's contention that the study of a formalized system of semantical rules, i.e., the study of pure semantics, will lead to the elimination of uncertainties, vaguenesses, and inconsistencies which beset all natural languages, and will thus clarify the fundamental meaning of 'meaning.'

In order to carry out his program, Carnap distinguishes between *object language* or the language spoken about, and *metalanguage* or the language in which we speak about the object language. He then defines a semantical system S as a system of rules, formulated in the metalanguage, which determine the "truth-condition" for every sentence in the object language. A semantical system, in other words, is an object language of such a type that every sentence in this language is interpreted or made understandable by a set of rules which specify under what conditions a sentence in S will be true. In such a system the meaning or sense of a sentence is fully determined by the rules which define its "truth-conditions." 'Truth-condition,' however, must not be confused with "truth-value." The former means only the general condition of relevancy to truth and falsity, while the latter indicates the specific value "true" or the specific value "false" for a given proposition.

A semantical system S which is developed in conformity with Carnap's requirements would consist of (a) *rules of designation*, defining 'designation in S'; (b) *rules of formation*, defining 'sentence in S'; and (c) *rules of truth*, defining 'true in S.' The definition 'true in S,' Carnap maintains, is the real aim of the whole system. Everything else is subordinate and preliminary to it.

The meaning defined in S is of a purely linguistic type. It is the meaning which is known to anyone familiar with the language and for which no knowledge of extralinguistic fact is required. There are no factual assertions in pure semantics. The rules merely lay down conventions in the form of a definition of 'designation in S'; and they define 'designation' by enumeration, thus: x designates t in S=Df ($x=in_1$ and t=Lincoln) *or* ($x=in_2$ and t=Omaha) *or* ($x=in_3$ and t=Des Moines) *or* ($x=pr_1$ and t=the property of being a state capital) *or* ($x=pr_2$ and *t*=the property of being a large packing house center).[2]

Following Carnap, but simplifying his symbolism and modifying his sequence of the rules, we now construct the semantical system K:

1. *Signs:*
   a. Individuals (in): 'A,' 'B,' 'C.'
   b. Predicates (pr): 'P,' 'Q.'
   c. Formal signs: '$\sim$,' 'V,' '(','.'
2. *Rules of designation:* a_i designates (an entity) u in K=Df a_i is the first and u the second member in one of the following pairs:
   a. 'A,' Lincoln
   b. 'B,' Des Moines
   c. 'C,' Omaha
   d. 'P,' the property of being a state capital
   e. 'Q,' the property of being a large packing house center.
3. *Rules of formation:* An expression E is a sentence (S) in K=Df. E has one of the following forms:
   a. pr(in)
   b. $\sim(S_i)$
   c. $(S_i)V(S_j)$
4. *Rules of truth:* S_k is true in K=Df one of the following three conditions is fulfilled:
   a. S_k has the form $pr_i(in_j)$, and the object designated by in_j has the property designated by pr_i.
   b. S_k has the form $\sim(S_i)$, and S_i is not true.
   c. S_k has the form $(S_i)V(S_j)$, and at least one of the sentences S_i and S_j is true.

Simple and limited in scope as this system is, it displays all the

[2] In these definitions, 'in_1,' 'in_2,' and 'in_3' indicate *individuals*; 'pr_1' and 'pr_2' indicate *predicates*; and '*t*' is a variable whose range of values includes both individuals and properties.

characteristics of a semantical system and, what is more, in its restricted scope it is complete and adequate and can readily be put to the test.

Let us examine, for example, the expression E_1: '$P(A) V \sim Q(C)$.' According to rule 3a, both '$P(A)$' and '$Q(C)$' are sentences in K. According to rule 3b in conjunction with 3a, '$\sim Q(C)$' is a sentence $\sim(S_i)$ in K; and according to rule 3c in conjunction with 3a and 3b, E_1 is a sentence $(S_i)V(S_j)$ in K. According to 4c, E_1 is true in K if and only if '$P(A)$' is true or '$\sim Q(C)$' is true or both are true. According to 4b, '$\sim Q(C)$' is true only if '$Q(C)$' is not true. According to 4a and 2, '$P(A)$' is true if and only if Lincoln is a state capital; and '$Q(C)$' is true if and only if Omaha is a large packing house center. It follows that E_1 is true if and only if Lincoln is a state capital or Omaha is not a large packing house center or both.

Several comments are in order.

(1) By adding other rules to the set given, a more comprehensive semantical system may be developed; and by changing the rules, different truth-conditions may be defined. But such modifications add little to the basic idea of pure semantics.

(2) By a proper selection of rules a system may be developed which corresponds closely to a natural language such as English. Admittedly, such a formalized set will be complicated and involved; but its possibility cannot be precluded on a priori grounds.

(3) As the example of K shows, the semantical system defines only the truth-conditions for any E in K, not the actual truth-value. For the determination of the latter, i.e., for the decision 'E_1 is true' or 'E_2 is false,' "facts" must be considered in addition to the rules of K. That is to say, we must transcend the field of pure semantics and must consult empirical evidence. Pure semantics, therefore, does not in itself solve the problem of truth (or falsity).

Another way of stating this would be to point out that a statement in the formalized language, say, $P(A) V \sim Q(C)$, is true if and only if a corresponding statement in the non-formalized language—such as "Lincoln is a state capital or Omaha is not a large packing house center"—is true. But whether or not the latter is true does not depend upon the rules of formalization.

(4) In the given set of rules for K, 4a seems to assume some form of a "correspondence" theory of truth. If, and to what extent, such an assumption is justified we shall consider in detail in Chapter IV.

(5) The set of rules for K shows that the "rules of truth" are independent of, and supplementary to, the "rules of designation" and the "rules of formation." This means that the truth-conditions disclosed in pure semantics are additions to the conditions of meaning. Since the "rules of designation" determine what corresponds to the *lexical* meaning of words, and the "rules of formation" determine *syntactical* meaning as we have defined it, the development of pure semantics corroborates our contention that meaning is independent of, and antecedent to, every truth-claim. All theories which define meaning in terms of truth-conditions overlook the fact that lexical and syntactical meanings are logically prior to all truth-claims, and that truth-claims can be advanced only because of that prior meaning.

(6) The introduction of "L-concepts" and "L-rules" which, for Carnap, paves the way to a semantic foundation of logic, does not alter the fundamental significance of lexical and syntactical meanings. This is especially true since any language which we employ in our analysis of "facts" must refer to non-linguistic designata. The crux of the matter, even for pure semantics, lies in the "rules of designation" or, to use more orthodox language, in the definition of terms. If our terms are well defined and if we observe the rules of syntax of the language we use, then the whole problem of meaning takes care of itself. The definition of terms, however, is not as simple as it appears to be at first glance.

The Problem of Definitions

Aristotle, metaphysician that he was, maintained that a definition should state the "essence" of the thing defined; that is to say, it should determine the timeless and universal concept of which the "thing" is a particular manifestation in the external world. This metaphysical orientation justified at once the specific form of the definition upon which Aristotle insisted; for the genus states the "essence," whereas the *differentia specifica* characterizes the specific form in which the "essence" is realized in the external world.

With the abandonment of Aristotelian metaphysics, however, the meaning of genus and differentia had to be re-defined. Sigwart, who in many ways gave impetus to modern logic, advanced the view that a definition simply fixates the meaning of a word, and that genus and differentia have no function other than to assign to a concept its proper place within an orderly system of concepts. But even this reference to systemic significance has been abandoned by writers who

maintain that "definitions are statements which give information about the meaning of words. They express the *association of a word with that which it means*."[3]

Since such diversity of opinion prevails it may be well to examine the matter once more in its broad ramifications (cf. Dubs).

In the first place, the linguistic formulation of ideas for the purpose of communication and the search for truth are two radically different processes and must be clearly distinguished. In the second place, thought moves essentially in the realm of meanings and ideas, not in that of words and sentences. Words and sentences are only the vehicles which we employ in thinking that which is other than the words and sentences which we employ, namely, ideas and meanings.

If these two assertions are true—and there is every reason to believe that they are—then a definition which has no purpose other than to fixate the meaning of a word in the interests of communication is quite different from a definition the purpose of which is to determine the content of a concept in the interest of truth. The former stipulates that a given word is to be used in a certain way, i.e., that it is to refer to or designate such and such (presumably known) referent; the latter delineates our comprehension of the referent.

If our sole concern is to indicate to a hearer (reader) the way in which we intend to use a word, definition by synonyms is perfectly adequate, provided the words used as the *definiens* can be assumed to be known. We may thus define 'acerate' by saying that it means *needle-shaped,* or 'predicant' by saying that it means a *preacher.*

If no synonyms are available, it may be possible to point out the intended referent and to say, for example, that the word 'typewriter' refers to or designates a thing such as *this* (indicating the "thing"). Definition in this sense is simply a matter of name-giving. Numerous elements of experience, such as the sensory qualia, can be defined in no other way. 'Blue,' for instance, designates the specific color quality of *this* cloudless summer sky at noon, of *this* flower, of *this* book-cover, of *these* eyes, etc. (and we try to get our hearer to *see* the color quality in question). We try to establish a direct connection between the "name" and that of which it is the name.

When direct demonstration is not possible, we may succeed in establishing the intended use of a word by enumerating instances or classes of instances to which it may be applied. We may say, for example, that the word 'mammal' designates *cows, horses, dogs, cats,*

[3] A. M. Frye and A. W. Levi, *Rational Belief,* 22. Italics in the original.

monkeys, tigers, rabbits, mice, and so on; or that the word 'spice' designates *pepper, nutmeg, cloves,* etc. In all such definitions there is no need for an explicit statement of genus and differentia.

Finally, we may determine the use of a word, or stipulate what it designates, by enumerating a number of attributes which (loosely) identify the referent. We may say, for example, that 'water' designates *the fluid which descends from the clouds in rain, and which forms rivers, lakes, seas,* etc. (Webster), or that 'person' designates *the real self of a human being* (Webster).

For purposes of communication such definitions are usually sufficient. Their adequacy (or inadequacy) is determined by the success, partial success, or failure in communication, and may vary with each speech-situation. They are in all cases explanations of how a certain word is to be used; i.e., they are forms of name-giving.

Now, if this were all there is to it, it would be difficult to understand the serious trouble encountered in finding "adequate definitions" in the sciences. But the trouble is genuine; and it arises from the fact that scientists are interested primarily not in the communication of ideas but in the comprehension of reality, the discovery of truth. "Adequacy" is here measured on a different scale.

The goal of scientific cognition is the development of an all-comprehensive, integrated system of propositions which are definite and unequivocal in meaning and which adequately disclose the nature of reality. The definiteness and logical integrity of such a system depend on perfectly determinate and unambiguous concepts and on the adequacy of such concepts for the comprehension of the "facts." Definitions in the various sciences, therefore, must determine the concepts in such a way as to make the integrated system of propositions or laws possible. They are the means of forming the materials out of which the system is to be constructed. They thus serve a function which is quite different from specifying merely in what way a certain word is to be used—although, of course, the determination of the concept also fixates the use of a word.

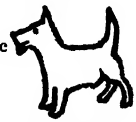
Let me try to make my point clear in another way.

It is possible to represent a definition schematically in the following way: N = Df f(a,b,c, ...). In this schema, 'N' stands for the word to be defined, i.e., it is the *name* to be attached to a certain referent. 'f(a,b,c, ...)' stands for the referent itself. 'a,' 'b,' 'c,' '. . . .' are variables representing different attributes. Now, if we are interested merely in determining the use of a word, we take more or less for

granted the unitary complex represented by 'f(a,b,c, ...)' and are primarily interested in associating with it the name 'N' which henceforth is to designate that complex. However, if we are primarily interested in comprehending reality, our chief concern will be an analysis of the complex indicated by 'f(a,b,c, ...).' Associating this complex with 'N' is of minor importance.

A simple illustration of this difference may be given as follows:

a. Definition for purposes of communication (name giving): 'Outline drawing of a Scotch terrier' = Df_c



b. Definition for purposes of truth finding: 'Outline drawing of a Scotch terrier' = Df_t, a drawing done in outline only, representing a dog of a breed originating in Scotland—a dog from 9 to 12 inches high, having short legs, a large head with small prick ears and a powerful muzzle, a broad, deep chest, a tail about 7 inches long, and a very hard coat of wiry hair, about 2 inches long, iron-gray, grizzled, black, or sandy in color.

Now, in connection with scientific definitions recourse was had to the term 'concept.' An explanation of this term will, I believe, throw further light upon the nature of definitions when definitions are employed as aids in our search for truth.

Consider for a moment the definition of 'water' given above: 'water' designates *the fluid which descends from the clouds in rain, and which forms rivers, lakes, seas, etc.*—i.e., N = Df f(a,b,c, ...), where 'a' = fluid, 'b' = descends from the clouds in rain, 'c' = forms rivers, 'd' = forms lakes, and so on. From the attributes mentioned we get a "general idea" of the referent called water. We can identify it sufficiently for purposes of communication. But in the deeper sense of scientific cognition we do not as yet know what water is.

Further observation will reveal additional attributes, such as "slightly blue in color," "boils at 100° C," "maximum density at 4° C," "consists of H_2O." Further observation, in other words, reveals a complex *aggregate of attributes*; but it reveals also that *not all of these attributes are essential* to the nature of water.

If we now discard unessential attributes and retain only those which are essential, the *concept* (as distinguished from a "general idea") emerges as *the totality of all essential characteristics of the object designated,* i.e., as an explicit reference to those characteristics which *indispensably belong together as constituting the object.*

The concept, therefore, is the result of a painstaking analysis of the complex designated by 'f(a,b,c, ...).' Its definition is an explicit

statement of the *essence* of the "thing" defined; and in this sense it is the "law" of the "thing."

If this is at all a correct interpretation of the meaning and significance of definitions in the service of science, then the definitions of a concept differ radically from all so-called definitions which merely specify how certain words are to be used. The former is an *analysis of the nature of a referent*; the latter merely *associates a name with the referent.*

"Essential" Attributes

The obvious objection to our interpretation of concepts and their definition is, of course, the old charge of an *implied metaphysics.* But this charge is ineffectual if it can be shown that methodological considerations alone suffice to determine what is and what is not essential, and that therefore the only metaphysics implied in our theory is the implied metaphysics of all cognition.

The separation of essential attributes of the referent from its unessential attributes presupposes a criterion of selection which, for Aristotle, was given in his metaphysics but which, for us, is not given in the same way. Is there a substitute for this criterion which is at the same time adequate and non-metaphysical and which satisfies all the requirements of modern science?

A first instance of such a criterion we find, I believe, in the field of jurisprudence. The subject-matter of this science, i.e., the "body of law" which regulates individual and social conduct, is an expression of the will and intent of the law-giver, i.e., of the legislature and, ultimately, of the people represented by that legislature. It is the function of the courts to apply the laws or to make the will and intent of the law-giver effective. Now, the application of the laws presupposes a clear understanding of the legislative intent and therefore requires "adequate" definitions. In fact, it requires that all "general ideas" be replaced by *specific and unambiguous concepts.* Mere name-giving—the association of a word with some referent—is not enough. The referent itself must be specified in its *essential* characteristics. Let me illustrate what I mean.

Consider for a moment the word 'person.' In general English usage this word designates *the real self of a human being,* without stipulating what this "real self" is. In the field of law this "general idea" of 'person' is of little use. In the first place, only that aspect of a "human being" is relevant to matters of law which pertains to his

social relations affected by legislative intent. This means that for all purposes of the law a man is a "person" only by virtue of his place in a society or, better yet, by virtue of the rights to which that place in society entitles him, and by virtue of the duties which it imposes upon him. This implies at once that not all human beings are necessarily persons in the legal sense; for some human beings may have no legal rights in a given society. In South Carolina, for example, a slave was considered a person only in so far as he was regarded as "capable of committing a riot in conjunction with white men" [State v. Thackam, 1 Bay (S. C.) 358]. In law, therefore, a person is such, not because he is human, but because rights and duties are ascribed to him by legislative intent.

Moreover, the term 'person,' as used in law, designates also certain "artificial beings," namely, the corporations; for under the law, i.e., in conformity with legislative intent, corporations have, in general, the same rights and duties as have individual human beings. They are "persons" within the statutes ***unless the intention of the legislature is manifestly to exclude them.***

Now, if, on the one hand, not all human beings are regarded as "persons," and if, on the other hand, "artificial beings" or corporations are so regarded, it is evident that the meaning of the word 'person' in the field of jurisprudence has been restricted to some attributes which are regarded as "essential" ***in relation to the legislative intent.*** And in this sense, stripped of all unessentials (such as reference to selfhood or to being human), a 'person' is the ***legal subject of which rights and duties are attributes.*** A human being is assumed to have rights from the moment of birth—unless disqualified by legislative intent; and a corporation has rights and duties (by legislative intent) from the moment its "charter" is granted. Both are therefore persons within the range of legislative intent.

Comment on this example of definition in the field of jurisprudence can be brief, for the implications are obvious. In matters pertaining to law, it is the "will of the law-giver," the legislative intent, which leads to the stipulation of the essential (as distinguished from the unessential) attributes of a referent. Legal concepts are defined for the purpose of expressing unambiguously and clearly the legislative intent. They are therefore defined in conformity with the prevailing philosophy of law and of society: Legal concepts, in other words, have meaning and significance only as elements within a system; and it is the system as a whole which determines their character.

Do we find comparable situations in other fields of human endeavor?

It is a well-known fact that in the field of classical mechanics the meanings of such words as 'force,' 'work,' and 'power' have undergone specific transformations. 'Force,' for example, in ordinary English usage means *power to influence, affect, or control things.* In this sense the term betrays its anthropomorphic origin; for "force," so conceived, appears to be something that is akin to the human will. In classical mechanics, however, the term, still somewhat loosely defined, came to designate *an action exerted by one body on another tending to change the state of motion of the body acted upon*; and, in a stricter sense, it came to mean such action *as measured by the product of mass times acceleration.* That is to say, 'force' was now defined by an equation: $F=Ma$; and this equation, as Newton's second law of motion, became one of the cornerstones of classical mechanics.

As far as classical theories are concerned, the term 'force' means only what this law expresses. Law and concept are essentially one and the same thing.

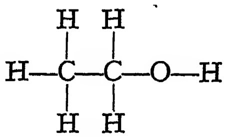
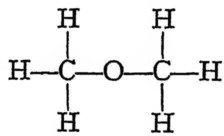
The terms 'work' and 'power' have undergone comparable changes of meaning. 'Work,' for example, in ordinary usage, means *something that is or was done*; and 'power' designates *the ability to do something or to act upon a person or thing.* These "general ideas," however, are too vague and ambiguous for the work of the physicist. He therefore re-defines them by incorporating in the respective concepts only those attributes of the referent which are *essential in the development of a system of interdependent laws.* Guided by the requirements of this projected system, he defines 'work' as *the product of the force times the distance through which an object is moved,* and 'power' as *the time-rate of doing work.* Both definitions eliminate what is unessential from the point of view of the system of classical laws, and both can be stated in the strict form of laws: $W=Fs$; $P=Fs/t$.

In electrodynamics and in relativity and quantum mechanics the equations are more complex but the principle of definition is the same. The idea of the system determines what is and what is not to be regarded as essential; and the fully defined concept is a law.

The same is true in the field of chemistry. Consider, for example, the terms 'ethyl alcohol' and 'dimethyl ether.' To say that they are compounds consisting of C_2H_6O tells only part of the story. This "empirical formula" discloses that, from the point of view of atomic theory, only the constituent elements of a compound are essential for

its definition. But evidently this information is not sufficient; for the same formula "defines" both compounds. The chemist, therefore, resorts to "structure formulae" in order to make evident the difference between the two referents.

Now, assuming the validity of the generally accepted valence theory, the empirical formula C_2H_6O yields two and only two stable structures, namely,

A:  and B: 

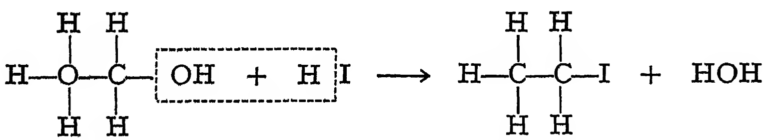
But which of these represents the alcohol, and which the ether? This question can be answered only by experimentation.

Out of the large number of reactions of ethyl alcohol we select one to illustrate the principle involved.

On application of moderate heat, ethyl alcohol reacts with hydrogen iodide in conformity with the following equation:

$$\underset{\text{Ethyl alcohol}}{C_2H_6O} + HI \longrightarrow \underset{\text{Ethyl iodide}}{C_2H_5I} + H_2O$$

This means that the iodide of the reagent displaces an oxygen atom and a hydrogen atom, i.e., that it displaces a hydroxyl group (OH) which appears in the molecule of water produced in the reaction. This suggests that the displaced elements were present in the original molecule *as a hydroxyl group*. Since only structure A contains such a group, the experimental evidence seems to imply that A represents the structure of ethyl alcohol. The reaction can then be represented thus:

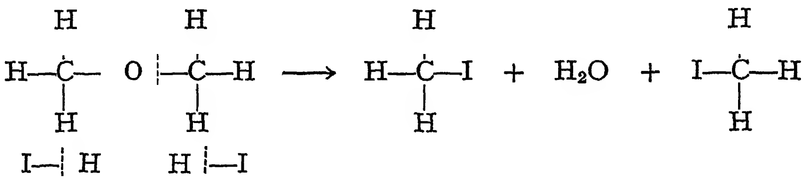


However, this interpretation can be accepted only with reservations until corresponding reactions of dimethyl ether have also been examined.

If this ether is made to react with hydrogen iodide, the result is quite different. It takes place in conformity with the following equation:

$$\underset{\text{Dimethyl ether}}{C_2H_6O} + 2HI \longrightarrow \underset{\text{Methyl iodide}}{2CH_3I} + H_2O$$

All six of the hydrogen atoms are retained in the two molecules of methyl iodide. This means that no hydroxyl group has been removed from the original compound and that therefore the reaction is not in harmony with structure A. Structure B, however, provides a perfectly natural explanation; for it implies that the reaction has taken place thus:



The hydrogen atoms of the reagent combine with the oxygen atom in the original compound, thereby breaking the connection between the carbon residues, which are now free to combine separately with the iodine.

Diagnostic tests with other reagents confirm this reactive difference between methyl alcohol and dimethyl ether. It is therefore beyond question that structure A represents the alcohol, and structure B the ether.

The same type of reasoning leads to the identification of structures for all chemical compounds. We can therefore generalize our interpretation. We find this to be the case: The name of a chemical substance—such as 'ethyl alcohol' or 'dimethyl ether'—is associated with a structure formula. This formula is the *concept*; for it specifies the essential attributes of the compound in question. 'Essential attributes' here designates structural composition *and* modes of reactions. The concept of a compound is therefore in all essentials the *law* of that compound. Knowing the structure formula, we know what type of reactions may be expected.

As in physics, so it is in chemistry: The idea of an integrated system of laws determines what is essential and what is not essential in the nature of the referents. *The idea of the system,* therefore, *determines the definition of our concepts.* I contend that, in principle, this is the case in all empirical sciences.

In most sciences we may as yet fall far short of the goal; but the goal is there. And it is basically the same in all sciences. It is: the

definition of concepts so carried through that these concepts specify clearly and unequivocally the (systemically) essential attributes of the various referents.

It is clear from the examples which we have discussed that the definition of concepts need not be given in terms of genus and differentia. Specifications of this type were the requirements of Aristotelian metaphysics. In the general field of the empirical sciences systemic considerations take the place of metaphysical stipulations. They determine the requirements of "essence" and, therefore, the type of terms in which our definitions of concepts should be given. Only one condition must be imposed upon all definitions, namely, that they be (systemically) adequate to the intended referent.

Definitions in Mathematics

On the face of it, definitions in the field of mathematics differ in at least one respect from definitions in the empirical sciences, for definitions in physics, chemistry, biology, psychology, and so on, clearly aim at a characterization of some phase or aspect of "reality" while definitions in mathematics aim at the delineation of ideal referents. The former, so it seems, are the result of a (systemic) analysis of observations while the latter are the product of a constitutive postulation. The former must separate the essential from the unessential attributes of a referent; the latter, because they *create* the referents in question, contain from the beginning only the essential attributes.

It is true, of course, that definitions in the empirical sciences aim at a comprehension of reality while definitions in (pure) mathematics deal only with ideal referents and have a relation to reality only when they are specifically applied as tools for analysis. But does this mean that definitions in mathematics constitute a type which is essentially different from that encountered in the empirical sciences? In my opinion, it does not.

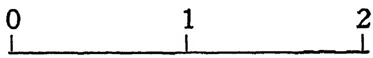
Consider, for example, such mathematical concepts as 'continuum' and 'parabola.' The word 'continuum,' in common English usage, designates *that which has contiguity of parts, that which is without break, cessation, or interruption.* But this "general idea" of a continuum is too ambiguous for purposes of mathematics. The question is, How can it be made more specific? How can it be restricted to the *essential* characteristics?

Because of the "threshold of our sensibility" neither our perception of spatial magnitudes nor our awareness of time intervals explains or

justifies the notion of a continuum. Actually, every attempt to "derive" the idea of a continuum from our perceptions by a process of abstraction leads into a vicious circle; for only the definition of the continuum can give meaning to whatever "continuity" we observe in space and in time. The definition of the continuum must therefore be obtained without recourse to sense-data.

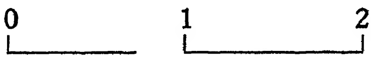
One way of obtaining it without circularity of reasoning is to define it as *the ordered sequence of all numbers* (irrational as well as rational)—as a sequence, in other words, in which there is *nowhere no number* (Dedekind's procedure); and then to stipulate that every other sequence which can be put into a one-to-one correspondence with the ordered sequence of all numbers is likewise a continuum. If this is done, the systemic significance of the definition is apparent; and its systemic significance alone determines what is and what is not essential to a continuum. Perceptual "continuity," for example, now turns out to be not only not essential but completely irrelevant. A simple illustration will make this clear.

The ordered sequence of all numbers from 0 to 2 can be put into one-to-one correspondence with a straight line



so that every number represents a specific point of the line. Perceptual evidence now shows that $\overline{02}$ is a "continuum." And since this line is of the same type of order as the sequence of all numbers from 0 to 2, it is also a continuum in the strict sense.

Let us now divide $\overline{02}$ into two subclasses, C_1 and C_2, so that C_1 contains all numbers from 0 to 1, but excluding 1 itself, while C_2 consists of all numbers from 1 to 2, inclusive. Let us assume, furthermore, that these two subclasses are given separately:



Perceptually, $\overline{02}$ is now no longer a continuum. However, the inherent order of the elements constituting $\overline{02}$ has not been disturbed; for within the class of elements defined by the order of $\overline{02}$—and these elements alone can legitimately be considered—the point 1, being the first element of C_2, is still directly preceded by all elements of C_1. That is to say, as an ordered sequence of relationally defined elements, $\overline{02}$ remains a continuum even after the cut at 1 has been made. The

law of the ordered sequence of all numbers is immune against all extraneous considerations; and this law, in all essentials, is the concept of the continuum.

Our second example, the concept of the parabola, need be discussed only briefly. *Webster's Collegiate Dictionary* defines the term as designating *the intersection of a cone with a plane parallel to its side*; and for many purposes this specification of *how to use the word* 'parabola' *correctly* is sufficient. However, the requirements of calculus and of other branches of mathematics are not satisfied by this definition. We must know not how to obtain a parabola but what are its essential attributes. And this demand for a specification of essential attributes is not satisfied by the dictionary definition. That definition gives us a "general idea" but not a concept of the parabola.

In the strict sense, a 'parabola' is *a curved line of such a type that every one of its points satisfies the equation* $y^2 = 2px$, where x and y represent distances in a system of Cartesian coordinates, and p is the parameter of the curve. This definition eliminates at once all unessential matters and specifies only what are essential attributes. But it does more. It reveals the concept of a parabola to be at heart a *law*—a law which attains its real significance from the integrated system of principles and laws which constitute pure mathematics. The concept, in other words, has an essentially systemic significance.

In all important respects the definition in the field of mathematics accomplishes what the definition of concepts in all other fields of cognition aims at, but which it does not always achieve, namely, the specification of the pure 'systemic' idea of the referent.

But enough has been said to make clear that there is no fundamental difference in the nature of the definitions of concepts in mathematics on the one hand, and in the empirical sciences on the other. In both cases we deal with definitions which aim at more than "name-giving," with definitions which specify the (systemically) essential attributes of the referents in question, and which express these attributes in the form of "laws." The concepts, strictly speaking, *are* the laws of the referents.

Definitions of this type are not just linguistic conventions which, like mere name-giving, are beyond truth and falsity, but are *cognitive* claims which, like propositions, are either true or false, or to which questions of truth or falsity are at least relevant.

Abstract and Concrete Universals

The issue of "concepts" versus "general ideas," as it emerges from the discussions of the preceding sections, seems to me to be essentially the same as that of "abstract" versus "concrete" universals—an issue which goes to the very heart of an old philosophical problem, but one which may also provide a test-stone for various "modern" philosophies. Let me try to state the issue clearly.

We have already seen that the use of words entails inevitably a recourse to "universals"; for name-giving involves both identification and differentiation—identification of the referent to which the name is applied, and differentiation of that referent from all referents to which the name is not to be applied. Name-giving, in other words, involves classification. It involves recognizing a specific referent as being a certain kind or type, as being an instance of a class—as when a certain animal is named 'cat' and another is named 'dog.' *The class name is the universal*, and *it is universal because it names a class.* It specifies or designates the attributes which the members of the class have in common.

Since *any* attribute may become the basis for classification, any attribute whatever may define a class, i.e., it may define a universal.

If this is true, then it is at once evident that not all universals so defined are of equal cognitive importance. The class of *mammals*, for example, has greater scientific significance than has the class of *blue objects*. But something else must also be noted—the fact, namely, that any given class may contain a subclass and may, in turn, be a subclass of some other class: . . . ., square, rectangle, parallelogram, quadrilateral, rectilinear plane figure, plane figure, figure, ...; . . . ., domesticated dogs, dogs, doglike carnivores, carnivorous mammals, mammals, vertebrates, chordate animals, many-celled animals, animals, living beings, ...; . . . ., light blue, blue, color, something seen, something perceived, something we are aware of, ... Such series as these reveal what in formal logic has been called the "inverse relation of extension and intension"; they reveal, in other words, that the range of application of a term is, roughly speaking, inversely proportional to its specificality of meaning; that in each case the more inclusive class B_1 is obtained when we omit certain attributes from the definition of the less inclusive class A_1 and retain only those attributes which are common to several classes, A_1, A_2, A_3, ... That is to say, such series of ever-expanding classes seem to imply (1) that universals are

formed by a process of omission and abstraction; (2) that the broader and more comprehensive a class is, the more devoid it is of meaning; and (3) that in each case a class is only a simplified "general idea" of a group of individuals.

Let us suppose for a moment that I observe a number of figures on a piece of paper. Upon closer inspection I discover that some of the figures are acute-angled triangles while others are obtuse-angled triangles and still others are right-angled triangles. Omitting the characteristics of acute-angularity, obtuse-angularity, and rectangularity, I now form the more general, the more abstract, idea of triangularity, i.e., I conceive the more inclusive class, "triangles." But then I discover that among the figures under observation there are some which are formed, not by three straight lines, but by four, five, and even more. Still, all of them are rectilinear figures. Hence, by omitting any specification as to the number of lines used in constructing the figures, i.e., by carrying the process of abstraction a step further, I form the new and broader class of "rectilinear figures." Further inspection of the figures on the piece of paper may reveal figures which are not composed of straight lines at all, namely, the curvilinear figures (circles, ellipses, etc.). Omitting all reference to the type of lines which form the figures on the paper but retaining whatever elements all of the figures have in common, I form the still more comprehensive class "plane figures"—a class which has thus been obtained by a continued process of abstraction and the definition of which is, correspondingly, empty of meaning. It is the "abstract universal"; but is it also the *concept* in the full cognitive sense in which we encountered it in the sciences?

The answer to this question is an unqualified No; and this No can be justified in a twofold way: (1) by disclosing the inadequacies and intrinsic difficulties of any theory of abstract universals, and (2) by giving a positive interpretation of the meaning of universals (Blanshard).

(1) The negative argument involves at least three distinct aspects of the doctrine of abstract universals. (a) A universal, if separated from its differentiations, loses all meaning. The universal "plane figure," in the example given above, was derived by omitting from its definition all reference to straight and curved lines. The "plane figure," in other words, is neither a rectilinear figure nor a curvilinear figure but just "plane figure." Actually, no such figure can exist nor can it be thought. A "plane figure" is a figure at all only because it

consists of lines in particular arrangements; and lines are lines only if they are either straight or curved. A "figure" composed of lines which are neither straight nor curved is an *Unding.* It is inconceivable. If we abstract from the lines composing it, a figure ceases to be a figure.

What is true in the case of "figures" is true, *mutatis mutandi,* in the case of all other abstract universals. As soon as we separate the universal from its differentiations, it becomes meaningless. The generalized and abstract "human being," for example, who is neither male nor female, neither child nor adult, neither white nor black nor red nor yellow, neither tall nor short, neither rational nor without reason, and so on, but is just "human being," is no more conceivable than is a "triangle" which is neither obtuse nor acute nor rectangular. A universal, in order to be thinkable at all, must remain inseparably intertwined with its differentiations.

(b) Abstract universals have little, if any, cognitive significance—a fact which becomes apparent the moment we try to apply the universal, i.e., the moment we attempt to get from the universal to its "species."

If a universal is the product of abstraction, then the transition to a species under that universal can be achieved only by the addition of attributes. And since the abstract universal does not define in a compelling manner what its species are to be, the addition of attributes, i.e., the definition of species, is casual and haphazard. In fact, the addition of *any* attribute will do so long as the added characteristic is compatible with the generalized universal. For example, in the abstract universal, "quadrilateral plane figure," there is nothing which compels us to retain as its species the "trapeziums" and the "parallelograms"; we can just as readily form the species "red quadrilaterals," "green quadrilaterals," "blue quadrilaterals"; or "large quadrilaterals," "small quadrilaterals"; or "quadrilaterals drawn on a blackboard" and "quadrilaterals drawn in my notebook." But this freedom from restraint is no aid to cognition.

After all, to be a trapezium or a parallelogram are *ways of being a quadrilateral plane figure*; to be great or small, or to be drawn on a blackboard or in my notebook, are not. The former characteristics are integral to the universal, the latter are external, or accidental, to it. To treat both types as if they were of equal significance involves a complete misunderstanding of the cognitive function of universals.

(c) The emptiness of the abstract universal corresponds to no emptiness of experience or of thought (cf. Blanshard).

If the theory of the abstract universal were correct, a wider range of application of a term should entail an increased paucity of meaning, an increased emptiness, because that wider range is obtained only by omitting restrictive attributes from our definition. But how different is our actual experience! Suppose, for example, little Robert gets his first dog, Spotty, a nondescript mongrel. An image of Spotty will then represent more or less completely Robert's idea of *dog*. But soon afterwards, Robert may become acquainted with Harry's fox terrier, or with Mr. Jones's German shepherd; and his idea of *dog* expands. He realizes that not all dogs have all the characteristics of Spotty.

As time goes on, Robert may come to know also a collie, a bulldog, and a setter; and if he takes an interest in dogs, he soon discovers that there exist greyhounds and pugs, St. Bernards and Pekingese, poodles and dachshunds, Scotch terriers and pointers, deerhounds and beagles, Dalmations and spaniels, great Danes and Boston terriers, wild dogs of the Orient and the dingos of Australia, and many other kinds of dog. As Robert's experience with dogs widens, he finds fewer and fewer attributes which all dogs have in common. Hence, if the theory of abstract universals were the whole story, the meaning of the term 'dog' would contract for Robert until it approached the vanishing point. Actually, however, the meaning of 'dog' is now for him incomparably richer than it was at the beginning; for he knows that the universal can manifest itself in all these differentiations. When he now speaks of dogs, he does not speak of a class of animals which is but a generalized idea of Spotty; he has at his command a ready reference to many varieties and species of dog, and to many individual dogs, and his universal 'dog,' as genus, is diffused with the knowledge of all these varieties and individuals. Compared to this richness and fullness of meaning the abstract universal is a vague and amorphous idea. It is at the most only a germinal thought which comes to fruition in and through the differentiation of species, and which comes to full realization only in the individuals. This, however, means that it attains full cognitive significance only as concrete universal.

(2) Whatever the nature of a dog may be, there is no abstract *dogness* which remains completely unmodified in the various species, and from which the species can be "derived" by adding, from without, new attributes to the original self-identical core. Similarly, there

is no abstract treeness, coloredness, squareness, or any other universal whose relation to its subordinate species is as casual and external as it would have to be if the doctrine of abstract universals were true. The universal, properly understood, is the *law* of the species, and the species are instances of that law. The universal is a *concept* in the sense in which we have previously defined this term.

What is meant here is probably most readily understood if we replace the essential negativity of the abstract universals by a positive interpretation. Thus, the meaning of 'triangle' is not an amorphous something which is *neither* obtuse-angled *nor* acute-angled *nor* right-angled, but it is a triangular figure which *must be either* obtuse-angled *or* acute-angled *or* right-angled. Where the abstract universal is essentially negation—an exclusion of particularizing attributes—the concrete universal is an affirmation of a particularizing and exhaustive alternation. The specifications of the different species are taken up into the very definition of the genus, and the genus is conceived only as the genus of such and such species. The genus, in other words, is defined in such a manner that it logically implies the species which are *modes of "being the genus,"* and no others; and no such implication is possible so long as we deal with abstract universals (cf. Blanshard).

To be sure, it is difficult to achieve adequate definitions of concrete universals; for they presuppose an exhaustive study of all relevant matters, a thorough and intimate comprehension of all the referents in question. Such definitions are therefore the goal rather than the starting-point of scientific inquiry. Their cognitive value is, however, correspondingly great; for an integrated system of concrete universals eliminates all elements of subjectivity from knowledge and discloses the essential attributes of all referents defined. The *differentia specifica,* and with it the conception of species, is no longer a matter of arbitrary choice but is determined in each case by the genus in question as *a mode of being that genus.*

The highest genus, *as concrete universal,* is the law of the whole of reality. Its definition is the ultimate but infinitely remote goal of *all* inquiry; and to its definition *all* branches of knowledge must make their contributions.

The Problem of Objectivity

The theory of concepts and of concrete universals here developed cuts the ground from under the old controversy between nominalists and realists and implies an idea of objectivity which finds special recognition in the various sciences.

Against nominalism (which is essentially a theory of abstract universals), the theory of concrete universals maintains that universals are not merely "names" but "laws" determining the character of the subordinate species; while against realism it insists that universals *exist* not in some special realm and by themselves but in the particulars and as the essential character of particulars. Both points are fairly obvious. The implied solution of the problem of objectivity, however, may require a somewhat fuller discussion.

All cognition, as a historical process, begins with experience and proceeds from a position best described as "naïve realism"—the position, namely, that real things are "given" in experience and are "given" *as they really are*; that knowledge consists in a generalized understanding of these things, and that the objective validity of judgments and laws stems from a correspondence between our ideas and the real things. Strict empiricism and all doctrines of the abstract universal derive their support from this basic contention and do not essentially transcend it. Yet, the position referred to is not only inadequate but false.

Let us examine one of the so-called "given" objects of experience. No matter how simple and unitary it appears to be at first glance, closer inspection reveals its complex nature; and the more closely we examine it, the greater the manifoldness of its attributes turns out to be. If the object in question is a *patch of color*, it possesses not only a distinctive *hue* (such as red or blue or green), but also a specific *tint* or *shade* (such as light blue or dark green), a particular degree of *saturation* (or purity), and a certain *brightness* (or luster). As "patch" it has *form* and *size* and is divisible, and it may be homogeneous (or heterogeneous) in all (or some) of its qualities in all (or some) of its parts. If it is the color of a "surface," still other qualities are involved, such as *roughness* or *smoothness* to touch, *warmth* or *coolness, resistance to pressure*, and so on. The list of attributes given here is suggestive rather than exhaustive; and we have not even referred to the manifold relations which this "patch of color" has to other "given" objects. Taking it all in all, the complexity of a "given" object is inexhaustible. No idea we have of it can be, in the strict sense, a *copy* of such a thing; and if it were, it would be as complex as the thing itself and would contribute little to our understanding of an infinitely complex world.

After all, cognition is possible only if we can simplify the complex manifold of experience and can reduce it to "law and order." Simplification, however, means selection; and this simplification through

selection is achieved through the use of concepts. Concepts, in other words, are not "copies" of things—not even "generalized ideas" obtained through the omission of "unimportant" details—but cognitive tools designed to establish law and order in the inexhaustible manifoldness of the "given," and thus to make that manifoldness intelligible and amenable to reason.

The simplification here referred to must be at least two-dimensional. (1) It must integrate into one unitary whole the various aspects of the same "given" as they are revealed in different experiential situations; and (2) it must bring different objects under the same concept. Happily, achievement of the former entails the latter; and both tasks are thus accomplished through the use of concepts.

In our discussions up to this point we have used the term 'object' rather loosely as designating something "given" in all its complexity. I fear that we have used the term as an abstract universal. Henceforth we shall mean by 'object' that *integrated unity of (actual or possible) experiences established through the concept.* The emphasis here lies upon "integrated unity"; for the object is no mere aggregate of attributes. It is, rather, a unity *necessitating the togetherness of these* and no other attributes. And in this sense, as defining the *necessity* of this unity, the concept is the "law" of the object—a rule of integration.

Our thesis is that every conceptual synthesis of experiential elements is based on the assumption that the synthesized elements *belong together necessarily,* and that the idea of this necessity is embodied in the concept; that it is the very essence of the concept. Concepts, therefore, are at heart judgments—judgments asserting a necessary interdependence of attributes. The concept 'body,' for example, asserts the necessary interrelation of "extension in space," "form," "size," "divisibility," "mass" (as distinguished from "weight"), "duration in time," and so on; and any object which can be subsumed under this concept must possess all of these attributes in addition to the attributes which are necessary characteristics of its specificality as *this kind* of a body. Concepts, in other words, legislate for objects.

The individual object would be of no cognitive interest to us if its concept were not a *law,* in the sense defined, *holding for all cases of its kind,* i.e., if the concept were not universally and unconditionally valid for the objects defined in it. However, not even such universality discloses the full significance of concepts.

If all concepts were strictly unrelated, we would still be in a pre-

carious position with respect to cognition. Not only must it be possible to form concepts which integrate and simplify specific aspects of the manifold of experience, but a system of interdependent concepts must give systemic unity to the whole of experience—or must at least aim at such unity. Ideally, there must be a "law" of laws, an ultimate concept which includes all others and from which all others can be derived as entailed consequences. The trend toward such a law (or ultimate concept) is quite evident in the sciences and finds general recognition in the ideal of "unified science."

All inquiry aims at "systemic unity" and cognition finds its rationale in this aim. In the sciences, the concepts themselves are defined with this aim in view; they are "pointed toward" the system.

That under these conditions the concepts of the various sciences bear the imprint of the system is not surprising. But this is a matter which we shall discuss when we deal with the special categories of the sciences; now our chief interest is centered around other matters.

We seem to have strayed rather far from the announced topic of "objectivity"; but what has been said so far should be regarded as introductory to the problem in question. It will then be found that our solution of the problem of objectivity is grounded in our theory of concepts.

The most difficult problem for empiricism is the problem of the validity of the "laws of nature," i.e., the problem of the objectivity of scientific knowledge; for empiricism, in the strict sense, is inseparable from the theory of abstract universals. Its "concepts" are obtained through the process of abstraction previously described. But upon this basis the "laws of nature" can be only empirical generalizations without compelling universality. The enumeration and comparison of a finite number of instances, i.e., the process of abstractive generalization, can never justify a universality which transcends the realm of examined instances. From the fact that some observed bodies fall in conformity with the equation $s=\frac{1}{2}at^2$ we cannot infer by abstractive generalization that *all* bodies *must* fall in the manner described by this *law*.

Moreover, the process of abstractive generalization itself cannot proceed arbitrarily but only according to some principle of selection which determines in each case of generalization why *these*, rather than some other, "common elements" are selected. Frequently, the principle guiding the abstractive selection is accepted as "self-evident" or is implicitly assumed rather than explicitly stated or recognized. More

often than not it stems from the pragmatic requirements of "practical living." For example, in this way is determined what constitutes a "thing" for the naïve realist.

Abstractive selection, however, *culminates* in classification, but classification is never the end or goal of science. That end is rather a system of universally valid and compelling laws. Classification is always arbitrary and, in this sense, subjective. Science, on the other hand, aims at universal validity, i.e., it aims at the elimination of that which is purely arbitrary in knowledge, and thus tries to achieve *objectivity*.

Since the method of abstractive generalization cannot achieve this objectivity, it has been assumed by some that the objective validity which science claims for its laws derives from the relation of our concepts to some "ultimate reality." Realists of various shades have made this assumption. For them, a correspondence between our ideas and the things as they are becomes the criterion of objectivity.

This view, however, also encounters insurmountable difficulties. Not only does it involve the well-known problem of how such correspondence can ever be detected, but it also fails to explain the nature of scientific knowledge. The "world without attributes" as disclosed, for instance, in pure mechanics is no longer a reality—ultimate or otherwise. The more fundamental and, in the scientific sense, perfect our concepts are, the less they contain of reality and the more they become implements for our intellectual mastery over the manifold complexities of experience.

But let us assume, for the sake of argument, that things *really consist* of "atoms" in the logical sense (we know that the "physical atoms" of chemistry are not ultimates). Even so it is obvious that we can obtain the concept of atoms only through an analysis, and interpretation presupposes certain principles of selection and simplification and involves concepts previously defined by means of these principles. Now, the validity of these principles must be assured before we can form the concept of atoms, i.e., before we can assert a correspondence between our thought of atoms and some *real* atoms. And if this is true, then it is vicious reasoning to base the validity of our principles upon the "real existence" of some "thing" or "reality" the very concept of which can be formed only upon the basis of those principles themselves.

If, nevertheless, we assume a connection between such a reality and the principles presupposed in all concept-forming, we are forced to accept a "pre-established harmony" between the two realms—a result,

incidentally, which leaves the problem of objectivity unsolved; for under the conditions of a "pre-established harmony" the validity of concepts and laws remains as mysterious as ever.

Objectivity, as aimed at in the sciences, is possible and perfectly intelligible on the basis of the doctrine or concepts developed in the preceding sections. All that is necessary for our understanding is that we disregard our (already discredited) naïvely realistic thought-habits. Instead of making the validity of our laws, i.e., the objective significance of our concepts, depend upon a correspondence with some assumed reality, we must realize that our idea of reality itself has significance only because it is based upon the objective validity of our concepts and laws. Science conceptually integrates experience in conformity with the laws and principles of thought which determine the formation of concepts, and the integrated system of the interdependent concepts implies and justifies our idea of reality.

Reality, at the beginning of our inquiry, is but a task, a problem, an X. It emerges at the end as that totality of actual and possible experiences which is integrated in and through our system of concepts. The objectivity and universal validity upon which science depends are the objectivity and universal validity of compelling thought as incorporated in our integrating concepts. Reality is the ultimate achievement of that thought.

In the chapters which follow, we shall carry through the ideas here indicated. In Part II, the idea of systemic wholeness and integration will be applied to the realms of experience and of truth, whereas in Parts III and IV it will be shown that the ideal of an integrated and closed system is the moving force and directive behind all formal and empirical sciences. It will thus be seen that one basic idea—the conception of systemic integration—provides the clue to our understanding of the basis and structure of knowledge.

PART II

TRUTH AND THE WORLD ABOUT US

CHAPTER III

THE WORLD ABOUT US

The thesis stated in the last paragraphs of the preceding chapter must now be demonstrated in greater detail. We begin with an analysis of that world-view which is experientially prior to all philosophical reflection—the world-view of the non-philosophical person, of the so-called "naïve realist."

Naïve Realism

As naïve realists we all believe that there exists a world about us—a world of mountains, lakes, birds, flowers, trees, houses, tables, books, and a myriad of other "things." We believe that we were born into this world, live in it for a number of years, and die; and that this world with its galaxies, streams, and forests, with its cities and hamlets, with all its beauty and tragedy, will continue to exist long after we have gone—just as it existed long before we were born. We believe that we can know this world, directly and immediately, as it really is; that we see it, hear it, smell it, taste it, and literally grasp it with our hands. Our faith in the reality of that world has been built up and supported by uncountable experiences from infancy on to our present state of maturity.

As children, we encountered this world in its most intimate relation to our appetites and desires. It was a world we could "finger" and "handle," a world of "things" which we could grasp and hold, possess or lose. But lost "things" we could find again; and the very fact that we could lose them proved that they were independent of our possessing them, of our grasping and holding them. "Things" were something which *might* be grasped or which *might* be lost, something, at any rate, which in some respects was beyond our control.

This "independence" of "things" characterizes reality not only for the child; it does so also for the unsophisticated adult, although the actual "grasping" may have given way to a "pointing" or "indicating" that *there* are the "things" which we regard as real.

As adults, however, we distinguish our ideas of things from the "things" themselves; and we say that these ideas exist only "in our

mind," whereas the "things" exist all about us in an "external world." In other words, we distinguish between an "inner" and an "outer" reality. We become naïve dualists.

This does not mean that the two realms are completely unrelated in our naïve world-view. Somehow they constitute a context of dependency—even though the nature of this dependency is not clear to us. We are sure that if there existed no real trees we would have no visual images or ideas of trees. In some way, we believe, the real "things" produce the ideas within us.

Now, within the context of the two realms—the inner and the outer—a "thing" is a special form of the real. It is that in which specific characteristics of the real are embodied. The "thing" *has* attributes. The attributes somehow *belong to* the "thing," and the "thing" is in some way the *bearer* of these attributes. The exact relation of "thing" and attributes cannot be defined at this naïve level of thought but this much at least seems certain: the "thing" and its attributes are inseparable. Think away *all* attributes, and the "thing" itself has disappeared.

Still, the "thing" is not identical with any one of its attributes, nor even with some specific group of them. It is, rather, the totality, the unified whole, and persists even when some of its attributes are changed. The "thing," in other words, is something lasting and permanent, something which may undergo changes and still remain itself.

Every change, however, is regarded as the effect of some cause, as the result of something which produces it—whatever the "something" may be. Hence, for the naïve realist, the reality of "things" is intertwined with the idea of effects. And, since "things" can produce effects, they can (and do) leave their imprint upon our senses. We see them, feel them, hear them, taste them, smell them; and our sense-impressions disclose the very nature of the "things." The rose yonder is as red or as white as we see it; this lump of sugar is as sweet as it tastes; and that fragrance of an orange is a real property of the orange itself. For the naïve and uncritical person this is all there is to the "mystery" of real "things."

Critical Evaluation

Occasionally our naïve faith is challenged; for occasionally our senses lead us astray or furnish contradictory data. What we regard as a man (when we see it at some distance in the fog) may turn out to be a tree (when we get closer to it). The peculiar specks which

move across the sky in a V-shaped formation may turn out to be wild geese rather than airplanes (as we first thought). Somehow *error* occurs in our dealing with "things," and its occurrence may puzzle us. If our senses disclose the very nature of "things," how is it possible that at times they lead us astray?

As a rule, our contact with "things" at the level of naïve realism is of pragmatic significance only. We want to use the "things" for practical purposes of living and "making a living." We therefore "correct" the error, i.e., we readjust our pragmatic attitude, and do not pursue the critical question. If, however, our interest is primarily in cognition, i.e., if we want to find out what really constitutes knowledge, the occurrence of error may arouse us out of our "dogmatic slumber."

Our encounter with illusions of various types may make us even more critical. Is the stick in the water really bent—as it appears to be? And if it is not, do my senses still disclose the nature of "things" as they are? At this point we may begin to regard our naïve faith with some distrust. We may begin to doubt the adequacy or reliability of an uncritical attitude.

Still, this doubt may at first involve only the validity of some particular cognitive experience, not of cognition itself. It may be necessary to "correct" repeatedly this, that, or the other specific perception of a "thing"; but the very fact that the correction is made shows an underlying faith in cognition as such. An error cannot be recognized as an error, or an illusion as an illusion, if we do not possess some standard of "true" cognition; and a correction of the error cannot be made, or an illusion allowed for, if we do not have faith in the reliability of our standard.

But whence do we get this faith? *How can we justify our standard? What is its warranty?* These questions bring to the fore the very crux of the matter. They cut the ground from under our naïve and uncritical attitude and make cognition itself a problem. Those who deny the significance of epistemological discussions have, I fear, never really faced these basic questions.

One thing, however, we must keep in mind clearly. The questions challenging the cognitive faith of naïve realism arise not because we have lost faith in cognition, not because we have despaired of ever knowing reality, but because we desire that our "faith" be no longer a blind "animal faith," a naïve and uncritical acceptance of the

"evidence of our senses," of the "givenness of reality," but rather a *warranted* belief; that it be knowledge rather than faith.

Our interest in the challenging questions may be motivated in different ways; but all of them center around the naïvely realistic contention that the "things" themselves provide the warranty for our faith, that the "things" themselves furnish the standard of true knowledge as distinguished from error and illusion, and that in perception we come to know these "things" as they really are. It is this idea of the standard of cognition which breaks down and becomes untenable under analysis.

Suppose, for example, we ask, Is the mountain we see over there the same mountain we saw here last year when we drove along this same road? The answer to this question, so it seems, must be an emphatic Yes. If we now ask, How can we be so sure? the naïve realist will, no doubt, be amazed at our question. And if we press our point he either dogmatically asserts that it "just *is*" or he explains to us that if it were not the same mountain, then a miracle must have happened, and that such miracles are impossible. But how does the naïve realist know that such miracles are impossible? Sense-perception alone cannot disprove them.

Now, the question we have asked concerning the identity of the mountain is neither idle nor meaningless; for if we had asked, Is the book we find on the table when we return to our room the very same book which we left there when we went out? the question would not seem so amazing. Somebody could have exchanged the book for another one during our absence. But both questions—one pertaining to the mountain, the other to the book—are fundamentally of the same type; for both are equivalent to the question, What assurance do we have that "things" remain what they are when we do not observe them? The answer to this question must of necessity transcend all perception; for even if we assert dogmatically and naïvely that they "just do," we already go beyond what we perceive.

What, incidentally, is meant by "the same thing"? If a piece of ice is melted and becomes water, if this water is boiled and becomes steam, and if the steam is dissipated and then frozen quickly to form the beautifully symmetric crystals of snow, are we throughout this process dealing with "the same thing"? Is the chicken still "the same thing" as the egg from which it was hatched, or the oak "the same thing" as the acorn from which it grew? Are ashes and smoke still "the same thing" as the wood which was burned? Are sense-percep-

tions sufficient to answer questions such as these? Or must we again have recourse to something which transcends mere perception? And if the latter, what is this "something" to which reference must be made? Is it the "thing itself"? Just what do we actually know about a "thing itself"?

The naïve realist maintains that "things" are as we perceive them to be. But, surely, this cannot be so; for the qualities of our sense-perceptions depend upon the conditions of our sense-organs as much as upon the nature of the "things." The very same substance, for example, may taste bitter as quinine to one person and be perfectly tasteless to another; and this difference in taste is a hereditary (Mendelian) trait.

Perhaps even more to the point are various modifications of the taste qualities. Sour-flavored candies (lemon drops, etc.), for example, taste insipidly sweet when eaten in the tropics (cf. Cricker). Is the same candy actually sour or is it sweet? Do raw onions and cinnamon possess the same taste quality because both taste sweet when our sense of smell is shut off? Do bicarbonate of soda, milk of magnesia, very ripe cantaloupes, clams, and shrimp possess in themselves and as "things" an "alkaline taste" quality despite the fact that this so-called "taste" is but a peculiar feeling in our mouth which results from slight injuries to the delicate mucous membrane due to the chemical action of the substance in question? In other words, do the experienced taste qualities actually disclose the nature of "things" as they really are?

Place a narrow "gray" strip of paper across a bright "green" background and cover both with thin tissue paper. The strip now looks "red." Is the strip itself *gray* or is it *red*?

We all know that a rose which is red in the bright light of day appears to be black in twilight. Is the *real* rose red or is it black? Or are red and black and all the other colors which we see experienced qualities which depend on many factors—such as the conditions of our color receptors and the conditions of light—and which can therefore not be directly ascribed to the "things" themselves?

If we chill our left hand by placing it on a cake of ice, and warm our right hand on a stove, and if we then plunge both hands into a pail full of lukewarm water, the water feels hot to the chilled hand and cold to the warmed hand; but is the water itself both hot and cold? Or must we give up the idea that our senses disclose the very nature of "things"?

Scientists have carried the analysis further and have taught us not

to accept the evidence of our senses at face value. The physicist, for instance, tells us that the pine tree in front of our window is not *really* there as we perceive it, in its beauty and fragrance. What is *really* "there," he contends, is a more or less orderly and coherent aggregation of molecules and atoms of an electrical nature, causing certain "ether vibrations" (or what not) to stimulate the retina of our eye. This stimulation is carried to the brain, so the physiologist tells us; and there, if we may believe the psychologist, a specific reaction occurs and we perceive the "pine."

In a similar manner, so we are told, we "perceive" sounds as our reactions to certain air vibrations which stimulate our auditory apparatus, and the taste of strawberries as our reaction to certain chemical actions upon the taste-buds of our tongue. The sensed quale of *red* "corresponds" to a physical wave-length of about 660 $\mu\mu$, that of *blue* to a wave-length of 440 $\mu\mu$ (1 $\mu = 1/1000$ mm; 1 $\mu\mu = 1/1000\ \mu$). The sound A_1 has as its "real" physical counterpart air vibrations of wave-lengths 772 mm. Salty taste is always due to ions, while sour taste is caused only by hydrogen ions. "Wave-lengths," "ionic states," etc.—such are the scientific counterparts of our sensory qualities; and in view of such facts, the naïve faith that perception discloses the nature of "things" as they really are, that it gives us an accurate and adequate "copy" of the attributes of "things," becomes untenable. All so-called "secondary qualities"—colors, sounds, tastes, smells, warmth, hardness, etc.—disappear from the world of real "things." There is left only a world of vibrations and whirling electrons—dark, colorless, silent—utterly different from our perceptual world. There remains only the world of "atoms," of "ultimates" whose only attributes are extension, shape, size, density, number, motion, and a cause-effect relationship—the world of "primary qualities" which science studies. But is this the *real* world?

Extension, shape, size, motion, and solidity, abstracted from all perceptual qualities, are inconceivable; and number and position are relative to the predilections of percipient minds. Hence, it would seem that neither the "secondary" nor the "primary" qualities can be attributed directly to the "things" themselves. But let us assume for a moment that the world of "primary" qualities *is* the real world—the world as it endures in time and as it exists in space. The question then arises, What are space and time? Are they, as the great receptacles which contain the world, also real? Are they entities of some sort, existing "things" in their own right?

Conceived as real entities, both space and time turn out to be impossible. They certainly are not "things" in the same sense in which mountains and trees and houses and stars and planets are "things," for they possess none of the attributes of "things" of this type. They do not consist of atoms and whirling electrons, of vibrations or energy emanations; they act upon no physical "things," nor do physical "things" act upon them; they do not enter into cause and effect relations with anything; they have no mass; nor are they impenetrable; we neither see nor feel them, nor do they offer resistance to anything. Hence, if they are entities at all, they must be entities of a peculiar type; they must be *sui generis*.

But is time an entity? If so, in what sense can it be said to be real? Surely, the past is gone and the future is not yet. If time is real at all, it can be so only now, at this moment, while it is still the present. But what is this "present"? It cannot be a finite "stretch of time"; for any time interval we may think of involves past, present, and future moments. Strictly speaking, therefore, the present is only that indivisible (because unextended) moment in which the future (which is not yet) becomes the past (which has been). Of the "time flux" nothing remains but this "now" which is the divide between two unrealities. It is a strange "thing" indeed that can have such an existence. Time, conceived as a real entity, as the "equable flux from eternity to eternity" (to use Newton's terminology), evaporates into nothingness.

And what about space? Conceived as a real "entity" it can be at best only an infinite "void" or infinite "emptiness"—a "void" because it must exist independently of all "things" that may be "put into it," and infinite because it cannot be bounded or limited in any sense or direction. It is inconceivable, however, that such an "inexhaustible infinity" actually exists as a completed whole; for it can be shown that the very idea of infinity is meaningful only as an unending progression or, rather, as an unending sequence of finite steps taken in strict conformity with a specified law of progression. Furthermore, if space is regarded as a real entity, it must at least be an *extended* "void." But if it is an extended something, does it not presuppose something else in which it can be extended—a "space" in which it can be an extended "void"? And if it does, what about this other "space"? Is it also a real entity? If it is, how can we avoid the infinite regress of "spaces" which contain "spaces" which contain space?

Lastly, there is no valid argument which would justify the conten-

tion that space and time are in themselves existing and real entities. Our senses disclose no such "things." Newton's arguments either rest upon premises which modern physics cannot grant, or they assume rather than demonstrate the point in question; and Euler's reasoning rests upon the assumption that classical (Newtonian) mechanics is the absolute and true interpretation of physical nature—an assumption which has been repudiated through the advance of relativity mechanics.

The only constructive and tenable interpretations of space and time all agree on this, that space and time are not entities at all but are objective orders of integration, that they are conditions of physical existence and of motion but that their objective character is a "construct" and that it depends upon the principles which we accept for the interpretation of experience. Is space, for example, Euclidean or non-Euclidean in character? It all depends on how we interpret the interrelation of "things" in space. We shall shortly dwell upon this point more fully.

Now, if space and time are not real entities but relational "constructs," what becomes of the world of real "things" which was supposed to exist in a real space and a real time? This world, as will be remembered, was assumed to be a world of *material* "things," of atoms and their configurations, of "things" which fill "space" and persist in "time." However, even in classical mechanics "matter" was largely identified with *mass* and mass with *resistance to a change in the state of rest or motion.* Mass or matter, therefore, was actually defined only in relation to "force." Faraday and Maxwell then introduced the conception of "fields of forces," and the "material" world was expanded in a direction still further away from the conceptions and tangibleness of immediate experience. Finally, as a result of quantum mechanical theories, the "substantiality" of the solid atoms was dissolved into "energy patterns," and even the electrons, protons, neutrons, and other "elementary particles" were reduced to "states of energy," to "phase waves" and "regions of maximum amplitudes of phase waves." In no sense, therefore, can it be said now that our senses disclose to us the nature of "things" as they are; and—what is much more important for modern philosophy—the whole idea that science discloses the nature of "things" upon which rests the objectivity, i.e., the objective validity, of thought is now so obviously wrong that one wonders how it could ever have arisen.

Every attempt to justify the objective validity of thought through an appeal to the "nature of things" as disclosed in the sciences involves a vicious circle; for the nature of those "things" can be known to us

only because thought, as employed in the interpretation and integration of immediate experience, is in itself objectively valid and compelling. There is no warranty for this objectivity except the nature of thought itself. And the "world about us," far from providing a secure basis for thought, emerges as an intelligible reality only because thought, in its objective validity and as employed in the integration of our immediate and first-person experiences, leads to the "construction" of such a world.

Methodological Solipsism

In Chapter I it was pointed out that whatever does not prove itself as warranted belief in my own first-person experience I cannot accept as knowledge, no matter what its knowledge-claim may be. In the preceding section we have seen that the objective validity of thought cannot be proven by an appeal to an existing real world but that, on the contrary, our knowledge of this world presupposes and is dependent upon that validity. Both these together imply (1) that only in our first-person experience can we discover the warranty for the objective validity of thought, and (2) that only through the employment of thought in the interpretation and integration of first-person experience can we come to know what constitutes the world about us. The "things" are not given but are conceived as the implied completion of an integrating and constructive process of thought.

If this is at all a fair statement of the broadest aspects of our cognition of "things," then, it seems, it suggests an approach to the basic problems of knowledge which may best be described as *methodological solipsism*. It is "solipsism" in so far as it starts with an analysis and interpretation of first-person experience—specifically, with an analysis and interpretation of *my* first-person experience, or of *yours* if you yourself carry through the integration. And it is "methodological" rather than absolute or metaphysical in so far as it implies no a priori and final restriction to the realm of first-person experience. On the contrary, it is hoped that all the elements of objectivity which carry us beyond the immediacy of first-person experience can be found within that experience itself; for, if they cannot be found here, they cannot be found at all.

The Starting-Point

All knowledge, we repeat, is rooted in, and grows out of, experience. Our view concerning the world about us—concerning its structure,

experience or must remain without justification. The word 'experience,' however, has different meanings for different persons. We shall use it here in the broad and all-comprehensive sense in which it designates *any* bipolar situation involving an "awareness," a "taking notice" of "something."

Awareness, in this sense, I take to be a state or condition *sui generis* which escapes formal definition. We know, however, what is meant from our own personal state of "being aware" or "taking notice" of something. When we hear a sound, see a color, remember a dream, have an illusion, or contemplate the future, we are "aware" and "take notice," and the situation is one of "experience" in the broad sense here intended. As yet no distinction is made between "things" and "ideas," between "reality" and "unreality," "rationality" and "irrationality"; and an "illusion" or "hallucination" has as much right to be taken into consideration as has a "perception" or a "sense-datum." Anything whatsoever of which we may be aware, or of which we may take notice, is an element in experience. No a priori restriction can be imposed upon the number or variety of the elements found in experience; for such a restriction would be a metaphysical prejudice. No selection of special types of elements can be made in advance of the complete analysis and interpretation; for any advance selection implies methodological bias, if nothing worse.

However, experience *is* bipolar. The something of which notice is taken is invariably and inescapably related to some "notice-taker." If awareness occurs, then there is a "something" which is aware of a "something else." We shall speak of the *subject pole* and the *object pole* of experience, but shall mean by these "poles" not entities which exist outside the bipolar complexity called experience, but relata within the encompassing whole known as experience—a "whole," which is what it is only by virtue of the unique awareness-relation in which subject pole and object pole stand to each other. What is ordinarily, and in the empirical sense, regarded as "subject" and "object" is the result of complex and augmented interpretations of the respective "poles," pointing far beyond the primordial experience-relation here intended.

Experience, as here understood, is a factum in the sense that it is implicitly and unavoidably present in every assertion we make, so that even its denial is proof of its actuality. Experience, in other words, is self-revealing. As soon as you affirm, doubt, or deny anything—and regardless of whatever else you may be—you are already one of the

"poles" within the bipolar manifold of experience of which that which you affirm, doubt, or deny is (part of) the other "pole." The empirical "self" or "person," however, is at this stage of analysis as much of a problem as is the empirical "thing" or the whole "external world."

Now, it so happens that the only "subject pole" of which we can be indubitably certain is the one which we designate by the first-person pronoun "I" and/or by its equivalents "me," "my," "mine."[1] You know yourself to be such a "subject pole" and I know myself to be one also. But you are for "me" part of the "object pole" of "my" experience, just as I am part of the "object pole" of "your" experience. No matter how intimate our relations to each other may be in other respects, as "poles" in experience we must remain eternally distinct as "subject pole" and "object pole," respectively, depending on who interprets his experience.

In the last analysis, therefore, we can proceed in our interpretation of experience only by inspecting what is the "object pole" in relation to the one "subject pole" we call "I." For the sake of brevity I shall call the experience centering in some specific "subject pole" a *first-person experience*—"your" first-person experience, or "mine," as the case may be. However, "I" can inspect and analyze only that first-person experience of which "I" am the "subject pole." Whatsoever does not constitute part of "my" first-person experience, or is not related to it in some definite and determinable way, must always remain unknown and unknowable to "me." This is true of "revelations" no less than of "sense perceptions" and of "logical deductions." Unless something is revealed to "me," or is communicated to "me" as having been revealed to persons of whom "I" have (or could have) some knowledge, the matter "revealed" can play no part in "my" knowledge; just as the sense perception which "I" do not have or of which "I" am not informed in some way cannot enter into knowledge as far as "I" am concerned. Of knowledge which is not "my" knowledge, which does not enter in some way into "my" first-person experience, "I" do know and "I" can know nothing.

A moment ago we said that the "I" which is the "subject pole" of "my" first-person experience—or of "yours," if you analyze "your" experience—is as yet not the empirical self or person ordinarily referred to by the personal pronoun, but is a relatum within the

[1] "I," "me," "my," "mine" (in quotation marks) will be used whenever reference is made to the "subject pole" of experience. The quotation marks will be omitted when the pronouns refer to the empirical self.

bipolar complex known as experience. One feature, however, is characteristic of this "I." The "I," as the "subject pole" of experience, is identical with what I have previously called "sign-consciousness." That is to say, "I" am not only aware of something or take notice of it, "I" can also designate or refer to this something by means of "signs" or "words." "I" can give names to (parts of) the "object pole" and can *mean* something through the instrumentality of "signs."

Hand in hand with "my" use of signs goes, finally, "my" discernment of discrete elements in the "object pole." "I" can and do *inspect* the "content" of "my" first-person experience and "I" can and do "take notice" of various distinctions.[2]

The First Elements of Objectivity

As "I" examine the "object pole" of "my" first-person experience, "I" am aware of "thoughts," "perceptions," "feelings," "desires," and other so-called "objects." "I" am aware of these "objects" in their variegated complexity and take notice now of "this" particular "object," now of "that." Let us assume that at this moment "I" take special notice of some one "object." It may be a "patch of green" or a taste of "bitter" or the "square root of 2." But whatever it is, this "object" is an integral part of "my" first-person experience, and it *is*, it "exists"—*at least for "me."* Whether or not it possesses any other "being" is immaterial for the present. Within "my" experience the object "exists" in the sense of "being there" as something of which "I" am aware, of which "I" take notice. And as such a "something" it is *self-identical*; it is a "this" to which "I" can attach a name.

Now, once "I" have given "this" something a name, "I" have assumed a compelling obligation. Either "I" abandon all hope of attaining a rational and intelligible interpretation of experience, or "I" adhere to "my" identification of "this" object through *this* name. To put it differently: Either "I" accept an utter irrationality and abandon all hope of ever attaining knowledge about anything, or "I" employ "my" words throughout any given argument or analysis with an identity of meaning.

That the alternatives are exhaustive and therefore compelling will readily be seen when a negative test is applied, that is, when the

[2] This "*in*spection" must not be confused with "*intro*spection"; for it is actually an observation and analysis of the "object pole" and not an attempt to "look into" the functioning of the "subject pole." The usual objections to introspection are therefore inapplicable to the *inspective* scrutiny of the contents of first-person experience.

attempt is made to think, not only coherently and rationally, but to think at all, while the meaning of the terms employed is constantly being changed. We simply have no other choice: Either we give up thinking or we accept the self-identity of meanings, i.e., the identification of "this" object as *this*, or of A as A. In the most elementary act of name-giving "I" must thus observe the "law of identity" or must abandon thought.

But as "I" single out "this" object from the rest of "my" experience, "I" separate it (at least in thought) from all other "objects." That is to say, "I" can speak of "this" object only as distinguished from some other "object" (or "objects") which "I" designate as "non-this" or as "that." "This" is not "that." A is not non-A.

Again "I" have no choice in the matter. If "I" want to think at all, i.e., if "I" accept the law of identity, "I" must accept also the "law of contradiction"; for this law is in all essentials only the negative application of the law of identity.

Once "I" have stipulated what a word shall mean, i.e., once "I" have decided to which object (or kind of objects) it shall refer and to what object (or kinds of objects) it shall not refer, then any object of which "I" am aware is either of the kind designated by the word or it is of a kind not designated by the word. It is either "this" or it is "non-this." It is either A or non-A. There is no third alternative. All "third" alternatives are excluded by the very nature of the stipulated meaning of the word in question. The "law of the excluded middle," in other words, is likewise an inescapable condition of thought entailed by the acceptance of the law of identity.

Our first steps in the analysis of first-person experience have thus led to most gratifying results. The traditional "laws of thought" have been uncovered as indispensable conditions of even the most elementary recognition of meanings. They are thus the *sine qua non* of all thinking. But they are also objectively compelling in the sense that we must accept them or cease thinking. And their validity is grounded in the nature of thought itself, not in some external agent.

However, these "laws of thought" stipulate purely formal requirements and conditions of thinking and, taken by themselves, are empty and barren. They are necessary but not sufficient for the complete integration of experience.

An indispensable supplement to the laws in question may be found in the basic "categories" which permeate and give "texture" to experience.

Dimensions of "Otherness"

As we have seen, taking notice of "this" particular content of experience involves at once and inescapably a discrimination between "this" and "non-this" or "that." It involves, in other words, a recognition of an "otherness" and, therefore, an awareness of a basic *relation*. This relation of "otherness" requires further analysis.

Red is "other than" *green*, and it is "other than" *orange*; but is it "other than" green and orange in the same sense? A *color* is "other than" a *sound*, and it is also "other than" a *taste* or an *odor*; but is it "other than" a sound, a taste, or an odor in the same sense in which *one color* is "other than" *another color*? A *sense datum* is "other than" a *feeling*, "other than" a *memory image*, "other than" a *desire*; but is the "otherness" in each case qualitatively the same? Or is it not rather true that the qualitative characteristics of the elements of experience which *are* "other" specifically determine in each case the very nature of "otherness" itself? "Otherness," I believe, cannot be understood when we regard it merely as "something common" to all cases of "being other," as "something left" when we have abstracted from all specificalities of the concrete situation; when we interpret it, in other words, as an abstract universal.

On the face of it, the complexity which we encounter in the analysis of "otherness" is bewildering rather than helpful. Further examination shows, however, that, if concretely interpreted, the qualitative differences involved in "otherness" become a first principle of order. For example, the very fact that *red* is "other than" *orange* in a way which differs from that in which it is "other than" *yellow* or "other than" *green*, enables us to place these various colors in a strict sequence of gradations—a sequence in which *red* fades into *orange*, *orange* into *yellow*, *yellow* into *green*, and so on. Similarly, the fact that sound A is "other than" sound B, in a way which is different from that in which it is "other than" sound C, makes possible the construction of a scale of sounds, A, B, C, . . . Again, the fact that the intensity of a pain L is "other than" the intensity of a pain M in a way that is different from that in which it is "other than" the intensity of pain N, and so on, provides the basis for a scale of pain intensities which, passing through a zero-point, finds its continuation in a scale of pleasure intensities.

It is evident, therefore, that the well-differentiated manifoldness of "otherness," positively interpreted, leads to specific sequences or

dimensions and, therefore, to specific elements of order in "my" first-person experience. This order is increased further when we realize that *colors, sounds, tastes, odors,* and other *sensory qualities* are "other than" *feelings of pain* or *feelings of pleasure* in a way which is different from that in which they are "other than" *memory images* and still "other than" *hallucinations,* or *dream images,* and so on. The "dimensions of otherness" previously referred to are now found to be interrelated in such a way that they constitute large "realms of othernesses." The whole of experience is thus permeated with these elements of order. It stands revealed as a manifold of specific "contents" which is yet not a mere chaos of unrelated "elements." First-person experience is stratified. It is "shot through" with the interlinked "dimensions of otherness."

If this is so, then certain *basic categories,* as the indispensable conditions of that order, can be found in first-person experience. We shall try to uncover at least some of them.

Basic Categories

The difficulties involved in any analysis of categories are formidable and the history of philosophy furnishes no ready-made solution for our problem. Not that the question of categories has failed to interest philosophers in the past. Part of the trouble arises rather from the fact that since the days of Aristotle so many great thinkers have meant so many different things when they discussed "categories" that the overabundance of meanings is bewildering. Of one thing only can we be certain at this stage of our analysis, namely of this, that it is immaterial for the present whether the categories are primarily those of "real things" imposed upon the "mind" (realism), or whether they are primarily those of the "mind" imposed upon the "things" (Kantian idealism). As yet we have encountered neither "things" nor "minds" which could *impose* categories. We are simply *inspecting* our first-person experience and are "taking notice" of what we find there. Our position at this time is antecedent to any metaphysical doctrine and is therefore "this side of" realism and idealism.

However, the difficulties of our task arise in part also from conditions inherent in the very problem of "categories," i.e., in the very problem of basic and irreducible concepts.

For one thing, we know of no "principle of deduction" (such as the Kantian, for example) by means of which a complete list of categories might be derived. We are confined in our search to the inspection of

the stratified content of our first-person experience and to a discernment of categories in the midst of the variegated manifoldness of that content. There is no a priori approach to our problem. Whatever knowledge of categories we obtain must be evolved out of the bewildering complexity of immediate experience by a process of painstaking analysis and a strict adherence to the "givenness" of certain characteristics of that experience.

Furthermore, the categories, once discovered, turn out to be so intimately interrelated that the meaning of one is fully understood only in interdependence with the meanings of all. Any "table" of categories is therefore an arbitrary arrangement. What we really uncover in our analysis is an *integrated complex of categories,* not individual categories or even "series" of categories. The table which follows must therefore be viewed with this idea in mind. It is at best only a tentative scheme which presents in linguistic separation ideas which are fundamentally inseparable.

Table of Categories

The basic categories discovered in first-person experience may be arranged in pairs as follows:

Quality—Quantity
Unity—Manifoldness
Form—Matter
Universal—Particular
Relation—Substratum
Dimension—Opposites
Continuity—Discreteness

These pairs and their interrelations require brief comments.[8]

(a) Quality—Quantity.—Of all the categories included in our table, the categories "quality" and "quantity" have received most attention in the past. This does not mean, however, that with respect to them all difficulties have been removed. On the contrary, most discussions of these categories proceed from metaphysical assumptions which can as yet play no part in our analysis. The resolution of qualities into quantities, for example, so strongly suggested by mathematical procedures in the natural sciences, transcends the realm of immediate experience and presupposes a world of realities which, for us, is as yet a problem rather than an established fact.

[8] For a radically different conception of the functions of categories see Nicolai Hartmann, *Der Aufbau der realen Welt.*

In first-person experience, the dimensions of "otherness" disclose most clearly what is meant by "quality." The experiential qualia red (*as red*) and green (*as green*), and both as different from each other in color, disclose *quality*; and so do bitter and sweet, sound and odor, warmth and pain, etc. And every one of these qualities is what it is in its experiential givenness, i.e., it is self-identical and irreducible, and it differs from qualities which are not itself. The category of quality, therefore, implies self-identity and difference as indispensable aspects. But where there is difference, there is also quantity; for there are at least *two* qualia, a *this* and a *that, one* and *another*, and there may be more.

We must not infer from this, however, that quality is the prior category upon which quantity depends. The question of priority (logical or otherwise) is completely irrelevant to the interrelation of categories. The category of quantity is no more dependent upon the category of quality than is the latter upon the former; for if there are two or more colors, two or more sounds, two or more tastes, two or more *this* and *that*, it is obvious that they must differ in quality. Every quantification implies a qualitative self-identity as well as a qualitative difference of the enumerated colors or sounds or tastes or "simples" of any type. Experientially, quantity is always a quantity of something; and in the simplest cases it is a quantity of some quale.

In its broadest and most elementary aspects the category of quantity implies the possibility of "counting." It discloses the enumerative aspects of first-person experience and implies the idea of "so many." But within the various dimensions of "otherness" the category of quantity also pertains to gradations of intensity and implies the idea of "so much." Quantity, in other words, may be of an enumerative or of an intensive type. In either respect it becomes the basis of mathematical interpretations of experience, and in both respects it interpenetrates with the categories of continuity and discreteness without, however, fusing completely with either.

(b) Unity—Manifoldness.—Quality and quantity, intertwined as they are, are also inseparably interrelated with "unity" and "manifoldness." On the face of it, the latter categories appear to be only forms of quantity; but that this is not so becomes evident when we realize that unity and manifoldness are interrelated in a non-quantitative manner. After all, the "simples" of experience are but abstractions. Even elementary awarenesses—such as those of color, of sound, of taste, or the like—are, within limits, complex. The "patch of red" I

am aware of is of *this* particular hue, of *this* particular shade, *this* particular intensity, *this* particular size, and *this* particular shape. It is a "manifold" of various characteristics. But as *this* particular patch of red it is also a unity of experience—a manifoldness *in* unity, or the unity *of* a manifoldness. Quality is here as much involved as is quantity. But neither quality nor quantity, nor both together, fully explain unity and manifoldness.

"Unity," by itself and separated from manifoldness, is barren and empty—a meaningless abstraction. "Manifoldness," by itself and separated from unity, is an incoherent chaos and a hindrance to all understanding. Only the two together, supplementing and balancing each other, are at all significant. Only in their interrelation do they become key concepts for the integration of experience. If taken together, they permeate the whole of experience from "sense-data" and "feelings" on up to "persons" and "historical events." Whatever else any particular content of experience may be, it is at least a manifold *in* unity, a unity *of* the manifold.

(c) Form—Matter.—In a sense, unity is *form* and manifoldness is the *matter* of a *this*; or, rather, every form is essentially also a unity, and the manifoldness of a *this* provides the matter that enters into its specific form. Nevertheless, unity and form, and manifoldness and matter are by no means identical; for *form* means not only unity of the manifold but also some specific arrangement of it, and *matter* implies not only manifoldness but something that is (or may be) arranged in specific ways.

Matter, in the categorical sense, is, of course, not *stuff* of any kind. It is simply that which enters into form; it is the counterpart to form, the residue we encounter when we break down a given form. It is therefore not paradoxical at all to find that all form is "matter" for some higher form, and that all matter is "form" of some lower matter. The "matter" of a particular patch of color, for example, is such and such a hue, such and such a shade, such and such an intensity, such and such size and shape. Its "form" is the specific combination of these "material" elements. For the experiential complex "rose," however, this patch of color, in combination with other qualia, is "matter"; the *form* is that of the "rose." In a bouquet of flowers, the "rose" becomes "matter" and the *form* is that of the bouquet as a whole; and so on.

The variety of forms is inexhaustible. Moreover, not all forms are

static. The form of a "living organism," for instance, is a *process form.*

The variety of matter is likewise inexhaustible. This follows from the fact that what is form at one level may be matter at another. Viewed superficially, it would seem that an analysis downward should sooner or later reveal the most basic, i.e., the undifferentiated, form of matter; but actually no such "matter" is ever encountered. A "sense-datum" is, after all, an entirely different "matter" than is a "dream image," and a "dream image" than a "feeling of pain." A color quale is matter of a different kind than is a sound quale; red is a different matter than is green; a light red other than a dark red; the low intensity of a light tint different than a high intensity of the same tint; and the intensity of a tint different than the intensity of an odor. No matter how far we push our analysis, the expected primary and undifferentiated matter still escapes us; for, at each level, matter is simply that which requires further analysis, and the sequence of levels points in diverse directions.

(d) Universal—Particular.—As *unity* of a manifold, any *this* may become a unit in enumeration; but as unity *of a manifold,* the *this* is more than an enumerative unit. It is a "particular"; i.e., it is a particular "something" possessing specific qualitative characteristics. The "particular," in other words, is distinguished not only by numerical singleness (Thomas Aquinas) but by qualitative uniqueness (Duns Scotus) as well. Every distinction of particulars involves differences in at least some of the characteristics; and any two particulars which in all respects possess the same characteristics are identical with one another. They are one and the same particular.

However, the "essence" of a particular, i.e., its determinate nature, is not a particular essence but a "universal." It exemplifies a class. Moreover, the elements of quality, quantity, unity, manifoldness, form, matter, space and time, etc., which constitute the essence of the particular, are also all universals. Hence, as long as we take notice only of the character or essence of "given" experiential complexes we do not really encounter particulars (cf. Blanshard).

Particularity is the uniqueness which a *this* derives from an interpenetration of *all* its relations to the totality of all (actual and possible) phases and aspects of first-person experience. The particular, in other words, is comprehensible only as an aggregate of specific universals. Its individuality depends upon a relationally determinate structure of reality of such a type that within it at least some of the

characteristics of the particular *this* cannot possibly be shared by any other *this*. Not until we have full knowledge of this structure as a whole and of all its interrelations and ramifications can we know individuality. Until we attain such knowledge we can understand so-called particulars only by subsuming them under various aspects of universality.

From this interrelation of universals and particulars stems the drive toward systemic completeness which characterizes all science and philosophy, and which is basic to our own attempts at integration; for it is only the system as a whole which renders fully intelligible the "particulars" of experience—the "particulars" which are, after all, only "intersections" of diverse universals.

(e) Relation—Substratum.—Where there is form, there are also *relations*; and where there are relations, there must be *substrata* as the indispensable relata of the relations; for it is obvious that relations cannot be relations of relations ad infinitum. Sooner or later relations must involve something which is not just another relation; and this "something" is what is meant here by a *substratum*.

The substratum, of course, is not matter; for matter, in the categorical sense, may consist of relations as well as of substrata. And least of all is the substratum "stuff" or "substance" (either material or mental). Actually, every dimension of "otherness" has the character of a substratum; for it is that of which a gradation is possible, that of which there may be more or less. The dimensions of space and time, of feelings, desires, sounds, colors, and so forth, are, in this sense, substrata; for they are the indispensable correlates of relations.

Relations are constitutive elements in experience; for all structure is relational, and so is all context. Some relations may be "external," i.e., their existence or non-existence may not greatly affect the specific character of a *this*. Other relations are "internal," i.e., they are essential for the specific character of a *this*. The difference between external and internal relations, however, is by no means clear-cut and absolute. It is, rather, a matter of degree. For the individuality and complete uniqueness of a *this*, all relations within the whole of experience are indispensable and therefore "internal." But for the practical purposes of science only relations of dependence are absolutely essential. All others may be regarded as "external."

Relations of some type are fundamental in all experience; for experience involves context and context is a matter of relations. All isolation of instances is secondary and the result of an abstraction.

Upon relations depend form, quality, quantity, and structure; and without relations there can be no unity of the manifold.

At least three types of relations may be distinguished: (1) relations which constitute "permanent" experiential complexes—such as are described by the "laws of nature"; (2) relations which determine individuality; and (3) relations which constitute broad realms of existence—such as "nature," "society," "history," etc. But this classification is suggestive rather than final; for the analysis of relations is only in its infancy.

(f) Dimension—Opposites.—The dimensions of "otherness," so we have said, constitute substrata of various relations. This does not mean, however, that the category "substratum" is identical with "dimension"; for the dimension is a correlate of "opposites" rather than of "relation." Any pair of opposites—such as light-dark, high-deep, pain-pleasure, love-hate, red-green, sweet-bitter—defines a dimension; and it does so even if the "opposites" are not end-positions. What is important is merely the "polarity of direction"; for such polarity is the essence of a dimension.

In all measurements, and therefore in all mathematical interpretations of intensities, such dimensions of "otherness" are presupposed as the non-mathematical substrata. Length, duration, velocity, weight, etc., are meaningless words without recourse to some substratal dimension—Bridgman's "operational definitions" notwithstanding.

In actual experience the dimensions of "otherness" occur only in interdependent "systems" or "clusters." In the case of a particular color quale, for example, the dimension of hues is interrelated at least with the two dimensions of shades and intensities, and probably with many more. Only through a process of abstraction can we therefore come to know a dimension all by itself and in complete isolation.

The multi-dimensionality of experience provides the basis for diversified classifications; for each classification rests upon a single dimension which has been selected relative to some specific purpose. The very fact, however, that one and the same *this* may be classified in different ways—as when a patch of color is classified as to its hue or as to its brightness or as to its shape or size or position in space—indicates the "intersecting" of various dimensions in this particular complex of experience.

(g) Continuity—Discreteness.—Every dimension of "otherness" is a continuum and, as such, is subject to unlimited divisibility. Divisi-

bility implies discreteness. Every dimension therefore also involves discreteness.

Experientially, discreteness seems to be most prominent and to be also a direct contradiction of continuity. Nevertheless, all discernment of differences (and such discernment is but the recognition of discreteness) is possible only upon the basis of continuity. Continuity, therefore, while not directly "given" in first-person experience—or "given" only within certain narrow limits—is a logical implicate of discreteness. It is actually the *conditio sine qua non* of discreteness. And continuity and discreteness, far from being contradictories, are, in this fundamental sense, complementary categories and are mutually indispensable.

The realm of discreteness is the realm of limited "objects"; but such "objects" are possible only because of the continuity of their constitutive relations within the whole of experience.

Spatiality and Temporality

The categories which we have just discussed permeate the whole of first-person experience and provide the basic elements of order which make an integration and interpretation of that experience possible. They are the key concepts of intelligibility. Whatever theory of experience or of reality we ultimately develop, if it is at all adequate to the totality of first-person experience, it must make some reference to quality-quantity, unity-manifoldness, form-matter, universal-particular, relation-substratum, dimension-opposites, and continuity-discreteness.

First-person experience, however, involves still further elements of order; for the whole stratified content of that experience is characterized by an experiential *temporality,* and large parts of it are characterized, in addition, by an unmistakable *spatiality.* Those experiential contents, in particular, which furnish the basis for our belief in an "external world" are invariably "given" in a spatio-temporal "setting."

To be sure, spatiality and temporality are basically matters of relation and are therefore, in a sense, included in the categories of order. They are, however, relations of such specific and prominent kind that they deserve special consideration. And, furthermore, they involve other categories as well as the category of relations. They are definitely specific dimensions of "otherness" and, as such, are substrata rather than relations. They are qualitatively different and are subject to quantitative considerations. Each represents a unity of a manifold.

Each is continuous and yet divisible; it is universal and can yet be particularized, and so on. Spatiality and temporality, therefore, are on a logical level other than that of the categories.

However, as found in first-person experience, spatiality and temporality must not be confused with "space" and "time" in any objective, i.e., physical or metaphysical, sense. Objective space and objective time, as specific orders of "things," are *constructs* involving much more than experienced spatiality and experienced temporality.

Experientially, certain components of the "object pole" appear to us to be "extended" and to be "placed side by side"; and this is just about all that we can say about spatiality as "given" in first-person experience. The rest is interpretation and interpolation. The three-dimensionality of "space," its "objectivity," its Euclidean or non-Euclidean character, its finitude or infinity, its homogeneity and infinite divisibility, and so on, are all matters involving highly developed mathematical and physical theories. But none of these theories would be particularly significant were it not for the primary form of spatiality under which some contents of experience are "given" to us.

What is true concerning "space" and spatiality is true also with respect to "time" and temporality. Time, as an *objective* order of "before and after," is a construct; i.e., it is the product of an interpolation of the temporality of experience.

In first-person experience we may be aware of such and such a sound and also of such and such a color or such and such a feeling or such and such a desire; and we may be aware of them all "simultaneously" or in "succession." Experientially, furthermore, some elements of the "object pole" may "endure" while others "change," "come into being," or "cease to be." Whatever our theory of the "specious present" may ultimately be, experientially at least such temporal features as "duration," simultaneity," and "succession" are undeniably real.

We hear snatches of "melodies," not merely isolated tones; and we see objects "in motion," not merely as occupying a spatial position now here, now here, and now here. And such experiences as these would be impossible were it not for an experiential "carry-over," for an experienced temporality which is not simply an unextended now-point. "Duration," "simultaneity," and "succession" characterize all our first-person experiences as they are "given" in the "specious present."

What "duration," "simultaneity," and "succession" are beyond this

experienced "present" is, however, a different matter and transcends the "givenness" of first-person experience. Is "simultaneity" absolute or relative? Are successive "time-intervals" equal in duration or not? Is "duration," as such, a metaphysical reality? Such questions can be answered only by elaborate theories based upon many and varied additional facts of experience; for the nature of objective time is other than the nature of experienced temporality.

Neither space nor time, in the objective sense, is directly "given" in first-person experience. Spatiality and temporality, however, are elements of order permeating the content of that experience, and they are supplementary to the categories previously discussed.

The Problem of the "External World"

We are now in a position to tackle anew the problem of the "external world."

Our problem is this: Is it possible so to divide or classify the "objects" of our first-person experience as to obtain a class of "objects" which are merely *contents of that experience,* and another class of "objects" which, *in addition* to being such contents, *also exist "out there" in an "external world" of self-sufficient "things"*—in a "world of things" such as the realist believes in?

The meaning of the term "out there" is as yet undefined and vague. It expresses primarily a certain feeling we have with respect to the "objects" in question, an "animal faith" in their existence as entities outside the knowledge relation. But unless this subjective "feeling" can be supplemented in some manner and can be supported by rational evidence, it falls far short of being an adequate criterion for the delimitation of the "external world."

Our problem is therefore to find a criterion (or a set of criteria) which will enable us to transform our "animal faith" into *warranted belief*, our blind trust into reasoned understanding or genuine knowledge, and which will yet be found exclusively in our own first-person experience.

Necessary but Insufficient Criteria

All objects of first-person experience which seem to be also "things" of an "external world" are spatially extended and stand in spatial relation to one another. "I" "see" them as so broad and so high; and "I" also "see" them side by side, above and below and to the right or to the left of one another. Spatiality, in the primary sense of an

element of order, thus characterizes the objects of "my" experience which are also "things."

But spatiality alone is not sufficient as a criterion. The objects "I" dream about also have spatiality; for "I" may dream about mountains and lakes, houses and trees, and landscapes of various forms. Afterimages, too, are so "broad" and so "high," and the objects of "my" hallucinations may appear "side by side." All objects of first-person experience which are also "things" in an external world are indeed spatial; but not all spatial objects are "things." Spatiality, in other words, is a necessary, but not a sufficient, criterion of "things."

Now, the objects of experience which are regarded as "things," in addition to being spatial, are also complex in the sense that they possess many attributes; and their attributes are primarily of a special kind. They are the colors and shapes "I" "see," the hardness and resistance to pressure "I" "feel," the tastes "I" "savor," the roughness or smoothness of surface and the hot or the cold which "I" experience. In brief, the attributes of these objects are primarily the so-called "sensory" qualities of experience.

But "I" cannot identify "things" and "sensory objects" without committing a logical fallacy. For if "I" consider only the experienced quale of so-called "sensory perception," "I" must admit that hallucinatory objects and afterimages are not without observable qualia; but still they are not "things." If, on the other hand, "I" maintain that in "sense-perception" the quale is experienced only in the presence and because of the "things" of the "external world," "I" assume the point at issue and argue in a circle. All "I" can say, therefore, at this point of my analysis is that wherever "I" deal with "things," specific instances of color, sound, taste, fragrance, and so on, are involved; but not every color "I" see, not every sound "I" hear, not every olfactory sensation "I" have, indicates the presence or the existence of a "thing." The problem of "things" and their existence is much more difficult than appears at first glance. This much at least is certain: spatiality and "sensory" qualities alone are inadequate as criteria.

The Emergence of a "Thing"

Suppose now that "I" "eat an apple," that is to say, suppose that "I" have all those first-person experiences which I ordinarily associate with "eating an apple." "I" then "see" a specific combination of colors distributed over a roundish area; "I" have "tactual" feelings of smoothness, and "kinesthetic" sensations of spatial expansion and

of resistance to pressure; and "my" visual and tactual experiences converge into a unique spatial pattern of distinct qualities. This pattern is further augmented by qualities of taste and fragrance, and by whatever else is qualitatively involved in "eating an apple." All of these qualities, however, are distinct; and all are, as qualities, independent of one another. The color is independent of the shape and, as color and shape, both are independent of the taste as well as of the fragrance and of all the other qualities. Nevertheless, all of these qualities are somehow integrated into a spatially extended and localized unity, and their togetherness constitutes the "apple" of "my" experience.

The question is, What brings about this integrated unity of the various kinds of qualities? Assuredly "I" do not infer it from the rational (or any other) nature of "myself" as knower; for "I" cannot deduce "sweet" from reason, "round" from desire, "red" from emotion, or the togetherness of "sweet," "round," and "red" from the manifold interrelations of reason, desire, and emotion.[4] Nor can "I" derive the integration of the qualities in question from the nature of those qualities themselves.

It is true, of course, that wherever there is color there must also be shape; and wherever there is shape there must be size. But such general interpenetration and interdependence of types of qualities have little, if any, bearing upon the specificality of the qualities which constitute the "apple." No matter how hard "I" try, "I" cannot infer from the specific nature of a taste that it must be associated with *this* particular color, with *this* particular shape, and with *this* particular resistance to pressure or *this* feeling of warmth. When "I" encounter a certain shape, "I" have no way of telling in advance and from the nature of the shape alone what color it must be, just as "I" have no way of telling from the nature of a "given" red what taste (if any) is associated with it. In "my" first-person experience "I" may encounter the various qualities in different combinations, and in any specific case of integration "I" may discover new and unexpected aspects and variations of the different types of qualities. In every case, the specifically integrated unity of the diverse qualities is definitely determined *for* "me," not *by* "me." It is determined in a way which is beyond "my" control.

[4] The "self," incidentally, which reasons, which has desires, and which is the subject of emotions—the empirical self as we ordinarily mean it—is as yet as much of a problem for "me" as is the simplest "thing" of the world about me.

Only in view of this "givenness" of the integrated unity of qualities can (and do) "I" speak of an "apple" (or of anything else) as a "thing" "out there" in a "world about us." Only in view of this "givenness," too, do "I" speak of the "substantiality" of "things."

But to say that a "thing" (the "apple" or whatever it is) is *that which possesses* these qualities does not solve our problem; for the "it" which is the "thing" in this sense is nothing apart from the qualities. If we think away all qualities, the "thing" itself, as the "it," disappears. "It" exists only in the unitary combination of the various qualities.

Nor will it do to argue that the bond establishing the experiential unity of qualities is a "common cause." For such an argument assumes either that we group the qualities together because we already know their common cause, or that the unitary complex of qualities is inescapably forced upon us by an as yet unknown common cause. If the first assumption is made, it is impossible to account for the fact that quite generally we group the qualities of a "thing" together as one complex unity long before we are able to conceive of them as the effects of one underlying cause. Or would you say that we already know the "common cause" of the fragrance, taste, color, and shape of an "apple"?

If the second assumption is made, we have no way of accounting for the fact that *these* and no other qualities are grouped together as constituting a "thing" because at any given moment of experience innumerable qualities of various kinds are "forced upon" us by many different "causes."[5]

In addition, the reference to a "common cause," however this cause may be conceived, transcends the realm of immediate experience and therefore can be of no significance at this stage of our analysis.

How, then, do we know what qualities "belong together" as the experiential unity of a "thing"?

Several criteria must be considered. First and foremost among these is the "joint prominence" of the qualities in question. The size and shape and the color pattern of an "apple," for example, are exactly coextensive and stand out together against a contrasting background. And the tactual qualities, the feeling of "smoothness" and of "resistance to pressure" are associated conspicuously with the size and shape and color (cf. Blanshard).

Since, in the case of most "things," the intimate association of visual

[5] Cf. B. Blanshard, *The Nature of Thought*, I, Chapter III.

and tactual qualities is especially strong and can be experienced repeatedly and at will, we regard it as particularly characteristic of "things." We forget that tactual and visual qualities belong to different dimensions of "otherness" and think of the "thing" as "extended," as "resisting pressure," and as "occupying the same space" as the color pattern.

The "factors of configuration" (as disclosed by Gestalt psychologists)—symmetry, simplicity, and *Praegnanz* of form—also contribute to our experiential groupings of qualities; for these factors favor certain types of pattern as against others, and they do so irrespective of "chance combinations," "mechanical stimuli," or "deliberate selections." Where alternative groupings are possible, the Gestalt factors, as a rule, determine the outcome.

Joint prominence and *Praegnanz* of form, however, imply as an indispensable condition that the association of qualities (which is, experientially, the "thing") persist in time. This does not mean that the combination must "endure" for a *long* time; but it does mean that it must be of sufficient "duration" in the specious present to be recognizable as a distinct configuration of qualities. No combination of qualities which does not fulfill this requirement can ever be regarded as constituting a "thing."

A further characteristic of all experiential complexes which are also "things" is the joint motion of their spatially intertwined qualities. That is to say, configurational complexes of qualities which move together as a unit are especially prominent as focal points of first-person experience. They attract our attention much more than does the unchanging background of patterns and forms. They, therefore, even more than the unmoving configurations, provide the experiential basis for our conception of "things."

Such joint motion, furthermore, suggests a certain *independence* of the experiential complexes in question. "Things" may move, or remain at rest, contrary to "my" expectation or "my" desire. They may move in a way over which "I" have no control, and may "appear" or "disappear" in a manner which is no more deducible from the nature of the experiential qualities or from "my" own nature than is the specific combination of these (and no other) qualities in any "given" case.

Joint prominence of qualities, configurational pattern, persistence of the pattern in the specious present, common motion of the interrelated qualities, and a certain element of independence of the inte-

grated whole—such are the characteristics of the experiential complexes which are also "things" in the world about us. But we cannot yet conclude that all experiential complexes possessing these characteristics are actually part of a "world about us"; for we do not yet know what it means for anything to be a part of an "external" world.

All that has been said so far concerning the particular characteristics of certain experiential complexes might well be true of the objects of our dreams and hallucinations as well as of the "things."

We must therefore augment the criteria just given so as to eliminate even the last elements of doubt. And this can be accomplished only by examining not isolated "things" but their interrelation and context; i.e., by examining the very meaning of "external world."

The World About Us

A moment ago we said that the qualities of sight and touch are especially important in our experience of "things." This suggestion deserves further consideration.

The reason for the particular prominence of the qualities of sight and touch is not difficult to find. The various colors seen at any time divide the visual field into distinct areas and provide the first demarcations for the delimitation of objects. The qualities of sight thus provide "me" with the first definite outline of what "I" come to regard as "things." Then, too, "I" can see the shape which offers resistance to "my" touch, or "I" can see the "distance" which "I" also feel tactually. The qualities of sight and touch, in other words, are so interrelated that they supplement one another in a specific and direct way. Their respective "fields" overlap, and both together form the basis for "my" outward projection or "externalization" of the "things."

The co-ordination of visual, tactual, and kinesthetic qualities discloses, furthermore, the three-dimensionality of the objects in question and makes it possible for "me" to view these objects not only in their immediate spatiality but also as unitary complexes existing in three-dimensional "space."

The attribution of "existence in space," however, by itself and in the simple form just stated, is evidently inadequate as a criterion of the world about us; for, while the objects of "my" dreams seldom possess the fixed order in space which "I" ascribe to "things," it is at least possible to conceive dream experiences which, as far as their spatial order is concerned, equal "my" experience of "things." If, in

"my" dream, "I" "walk across" meadows or climb mountains, "I" may note perspective changes in "my" dream objects which correspond to the perspective changes which "I" would observe under similar conditions while "fully awake." The fact that the "space" in which "I" encounter the objects of "my" dream is not continuous with the space in which "I" encounter the "things" of the external world does not in itself prove the "reality" of the latter and the "unreality" of the former—although it may indicate the general direction which "my" argument must take if "I" am to justify "my" belief in an external world of real things.

Now, the "things" in space which comprise the world about us are not merely spatially extended and spatially related; they also *occupy* space. Each "thing" which is located in some part of space prevents other "things" from occupying the same part. If "thing" B is moved into a part of space occupied by "thing" A, then A must be pushed out of that part of space. If A remains where it is, then B cannot move into that place. That is to say, by virtue of the fact that they exclude one another from the parts of space they themselves occupy, "things" "act upon" and "affect" one another.

This influence which "things" have upon one another—their incompatibility in the same part of space—is, however, only one of the many relations between them which can be discovered. A "stone" exposed to the "sun" gets warm; "fire" changes "wood" into "smoke" and "ashes"; a "storm" uproots "trees," and in the presence of "soil," "moisture," and "sunshine," a "seed" develops into a "plant." In all these instances "I" encounter relations which cannot be deduced from merely "subjective" factors. They are not formal relations of logic or cognition, but are relations involving deeply the very nature of "things." Only an analysis of the "things" themselves, therefore, can reveal their interdependence; and, conversely, only an analysis of their interdependence can truly reveal the nature of "things" as *things*.

"I" know what "fire" is partly because "I" know that it changes "wood" into "smoke" and "ashes"; "I" know what a "seed" is because "I" observe that, in the presence of "soil," "moisture," and "sunshine," it develops into a "plant"; and "I" understand what "hydrogen" is from observing what it will "do" in the presence of other "elements." In general, "I" comprehend any given "thing" only by observing its various relations of interdependence with other things," i.e., by viewing it in a coherent and continuous context of interdependent complexes of experience. This context—added to all

the criteria previously mentioned—distinguishes the "things" of the "external world" from all other objects of experience; for this context is the very essence of the "external world" itself.

The objects of "my" hallucinations and dreams simply do not fit into this context of interdependence and continuity. The millions of dollars "I" own in a dream will not buy the next morning's breakfast, and the snakes seen during delirium tremens do not disturb a blade of grass in the world of "things." Nor can "I" transport myself from the space of "things" into the "fluid" spaces of dream objects by a continuous motion in some particular direction—not even in principle is this possible. The "transition" from the space of "things" into the spaces of dream objects can be accomplished only by changing the status of "my" experience itself, i.e., by falling asleep and dreaming.

Of course, if "my" various dreams (or hallucinatory experiences) were such as to constitute one coherent and continuous pattern of interrelated events, while "I" would always "awaken" in a new and strangely disorderly environment, there would be no reason why "I" should regard "my" dreams as "dreams" and not as segments or parts of a "real" world. If the objects of "my" dreams (and of "my" hallucinations) should reveal throughout the same coherency, continuity, and interdependence which "I" observe in the case of real things, "I" should have to ascribe to them the same type and degree of reality which "I" ascribe to "things." "Fact" and "fancy" would in that case be as indistinguishable for "me" as they are for some primitive men, for children, and for lunatics.

When we come right to the point, we discover that continuity and coherency of context and a mutual interdependence of the "things" are the only criteria which enable us to delimit the world about us and to exclude from that world the objects of dreams and fancies. If this criterion fails, we have no other.

The situation, however, is most encouraging; for "my" dreams and hallucinations are discontinuous and incoherent in their sequences, and the objects of these dreams and hallucinations do not form a pattern or context of interdependent events. On the other hand, ordinary experience and scientific inquiry reveal ever anew the essential interdependence of the "things" of the external world and disclose the coherency and continuity of the pattern of "things." Every advancement in science produces new evidence of this context and gives "me" new assurance that the objects of "my" dreams and the objects of "my" hallucinations are not part of the pattern called

"nature." The better "I" understand this pattern, and the more fully "I" grasp its coherency, the easier it is for "me" to exclude the objects of dreams and hallucinations and of other normal or abnormal "fancies" from the realm of "things."

"Things," "Constructs," and the Pattern

The pattern of "things" which is the world about us is, in a measure, discernible in first-person experience. Were it not so, we should never know anything about an "external" world. This does not mean, however, that the pattern of "things" is "given," at once and as a whole, as the object of an immediate awareness. On the contrary, the pattern itself is only an "implicate" of the various elements of order which characterize first-person experience. While its broad outlines may be fairly obvious, its details and ultimate structure can be uncovered only through painstaking analyses and through careful adjustments and interpolations of diverse traces or fragments of order.

However, adjustments and interpolations of immediate experiences are involved from the very moment we begin to regard certain complexes of interdependent qualities as "things"; for even simple "things" are not "given" in their entirety at any one moment of awareness. The "sheet of paper," for example, which "I" now see has "another side" which "I" do not see at this time; and the "apple" has an "inside" which is not "given" in experience as long as "I" see only its "surface." In any experience of "things," therefore, "given" qualities and interpolations of "non-given" elements, or interpretative judgments, are involved.

The relative proportion of "given" elements and interpolative judgments varies from case to case. When "I" merely identify some complex of visual and tactual qualities as being a "piece of paper," comparatively little interpolation is involved. If, on the other hand, "I" identify a luminous patch in the sky as "Encke's comet," interpolative judgments predominate; for such an identification cannot be made without recourse to astronomical theories concerning the nature and orbital motions of comets, nor without referring to the specific characteristics of Encke's comet.

If I hold a burning match to a "piece of paper," the paper will also burn. "I" can observe how the flame "consumes" the paper until nothing is left but a bit of "ashes." Before my very eyes, so to speak, the qualitative complex "paper" is gradually transformed into a quite different qualitative complex "ashes." "I" observe, directly and imme-

diately and in the specious present, this transition. But suppose now that after setting the "paper" on fire I leave the room or close my eyes or keep in some other way from observing directly what is going on. If, after a while, I look again at the place where "I" last saw the burning "paper," "I" now see some "ashes" instead of the "paper." Remembering "my" earlier experience of observing the transition from "paper" to "ashes," "I" now interpolate "my" new observations and maintain that, even when "I" was not aware of the process of burning, the transition from "paper" to "ashes" went on as before. "I" justify this interpolation (and others like it) as contributing to the orderliness of the over-all pattern of "things," i.e., as contributing to the continuity and coherence of that pattern and to the schema of interdependent "things" which "I" have accepted as the criterion of the "external world."

The same line of reasoning leads to the stipulation that there exist "things" of which "I" am in no way directly aware at this time but which are "potential" objects of experience for "me." The "Grand Teton," the "Nebraska State Capitol," the "book on the desk at home," the "engine in my car," and a myriad of other "things" belong at this moment to the class of "potential" objects (as distinguished from "things" which are now actually present in "my" experience). The pattern of "things" as a whole would be different from what it is were these unobserved "things" to be ruled out as non-existent.

And what is true in the case of unobserved "things" which yet may become objects of direct experience, is true also in the case of those "things" which belong to the historical past, i.e., of those "things" which have "ceased to exist." The pattern of "things" as a whole, and as "I" interpolate it at present, would be different from what it is if its continuity and interdependence did not imply the past reality of the now-non-existent "things." It is again the pattern of "things" which entails their previous existence.

Finally, for the sake of order and in the interest of the pattern of "things," "I" may find it necessary to introduce certain "constructs" as indispensable supplements to (potentially) observable "things." The demands for continuity of the pattern and for a thoroughgoing interdependence of "things" may (logically) compel me to include "fields of force," "atoms," "electrons," "energy levels," "phase waves," and other non-observable "entities" in the pattern of "things"; for without such "constructs" the order of first-person experience would remain haphazard, fragmentary, and limited in scope and validity.

It is the ideal of the pattern, in other words, which ultimately determines what shall and what shall not be included in the world of "things."

We have, however, no a priori knowledge of the nature of the pattern in its completeness. We can only construct it on the basis of the fragments of order encountered in first-person experience. And it is this fact which makes the task of scientists and philosophers hazardous and difficult; for in our interpolation of experience we must imaginatively anticipate what the pattern might be, and then, turning about, we must interpret and integrate our experience in the light of that anticipated pattern. Only a painstaking process of critical readjustments of our interpolations and applied patterns over a long period of time can ever assure us of ultimate success, i.e., of discovering a pattern which will integrate the whole of experience without contradictions and gaps, and which will fully account for all "things" as indispensable elements of the pattern itself.

My Self

So far we have been concerned almost exclusively with the "object pole" of first-person experience and have neglected its "subject pole." This "subject pole," however, the "I" of first-person experience, also requires interpretation—especially since various questions concerning it must already have arisen in the reader's mind.

It will be remembered that we started out at the beginning of this chapter with the statement that "I" am the "focal point" of "my" first-person experience, that the objects of this experience are all somehow related to "me," and that nothing which is not found in "my" first-person experience or is not in some way related to it can become known to "me." Throughout our discussions we have used the grammatical symbols of the first person singular to refer to the subject pole of first-person experience, and we have employed quotation marks to indicate that nothing but the subject pole was intended. But this reference to "I," "me," and "my" has unavoidably suggested a simplicity and singularity of the designatum which is far from the truth. Is, for example, the "I" which now hears a particular sound the same "I" which yesterday experienced the pain of a "toothache" and which the day before yesterday dreamed about a sojourn to Antarctica? Is the "I" which experiences pleasure and desires some particular object still the same "I" which perceives and thinks, which hopes and regrets? Is the "I," as such, something which persists in time, a unitary entity

which endures in the midst of all change? Or is it a sequence of ever-changing combinations of awarenesses, a "bundle of distinct perceptions," as Hume maintained?

The dimensions of "otherness" imply that the "I," whatever it may ultimately turn out to be, is experientially correlated with diverse types of qualities. "I" *see* colors and shapes; "I" *hear* sounds; "I" *taste* flavors, *smell* fragrances; "I" *feel* warmth and, in a different sense of the word 'feel,' "I" *feel* pain; "I" *think* thoughts and *hate* vice; "I" *anticipate* the future and *recall* the past; and "I" have other modes of experience as well. It is easy to infer from these facts that the "I" which has all of these experiences is but a specific collection or set of events and has no existence apart from and beyond these various awarenesses themselves; that, as a matter of fact, there is no enduring "I," but only the sequence of interrelated experiences as such. I believe, however, that this "serial theory" of the "I," plausible as it may seem at first glance, entails insurmountable difficulties and that it must be abandoned if a really coherent integration of first-person experience is to be achieved.[6]

To begin with, let us suppose that "I" see a color or hear a noise or desire an apple or believe a proposition or have any other kind of experience. In other words, let us assume that "I" am aware of "something" as a specific experiential quale or as a specific complex of qualia. Each instance of "being aware of something" I call a "mental event"—using the term "mental" as indicating the nature *sui generis* which distinguishes an "awareness" from a physical "thing," but without committing us to any metaphysical theory of "mind." If this terminology be accepted, then it is tautological to say that wherever and whenever "I" experience anything, "mental events" occur; for "being aware of something" is equivalent, on the side of the subject pole, to the "occurrence of a mental event."

My contention is that wherever there is an "I," there occur "mental events" either in sequence or in simultaneous correlation or, as a rule, in a complex interrelation of sequence and correlation. Moreover, wherever there exists such a sequence and correlation of "mental events," there will also be found a "biography," i.e., a set of "mental events" so interrelated that "I" know them all as "my" experience *now*, that it was "I" who had such and such other experiences *before*.

[6] It should be noted that our criterion of evaluation is once more the ideal of a coherent pattern of interdependence, and one which is in harmony with the pattern of the world of "things." No other criterion can be found.

Any theory of "self" must give an adequate interpretation of both, the complex interrelation of "mental events" and the "biography" of the "I"; and this the "serial theory" fails to do (cf. Gallie). But let me be more specific.

In the first place, every "mental event" is essentially complex and relational: "I" am aware of "something"—be it a sound, a color, a dream-image, or a "thing." A "mental event," in other words, consists of a relation between "something" and "something else." Now, we know from our analysis of categories that the relata involved in relations must ultimately be substrata of some sort rather than merely other relations. We know, furthermore, that the object-side of the awareness-relation is constituted by the various dimensions of "otherness," by the different qualia and by "things." But what constitutes the subject pole? In view of the nature and interrelation of categories, the subject pole cannot consist of relations alone but must involve a substratum of some kind. We can let it go at that, for the present.

Let us consider next the fact that the "serial theory" of the self assumes that there exist or occur particular "mental events" and that different "mental events" are interrelated in complex series, thus constituting the "I." Two questions are in order: (1) What is a particular or single "mental event"? (2) What justification is there for maintaining that the "biography" of the "I" involves nothing but a collection of such events?

Turning to the first question, let us assume for a moment that "I" hear a snatch of melody or see a "piece of paper" burn to "ashes." Is either of these experiences a single "mental event"? Clearly, the flickering of the flame and the crumbling of the paper as it turns black could be broken down into distinct sequences of specific experiences which differ from moment to moment as the burning proceeds. Each momentary experience involves a substratum on the subject-side; but would you contend that each involves a different substratum? If so, how can we account for the fact that the burning of the paper or the snatch of melody is experienced also as a unitary whole occurring in the specious present? Does this involve a still different substratum, one which overlaps all the others and is yet not identical with them? Is there no escape from the multiplicity of substrata? Does the infinite divisibility of an experience in time imply an infinity of "I's" as the substrata of the infinite number of "instantaneous" experiences thus creatable? Or does it not contribute much more to the order and intelligibility of experience to hold that the temporal and qualitative

continuity of specific experiences has as its correlate one single substratum which persists in time?

Let us assume next that "I" see the colored shape of an "apple" at the same time that "I" feel its resistance to pressure and the smoothness of its surface. Is the "I" which *sees* also the "I" which *feels*? Is the experience involving such diverse qualities as those of sight and touch correlated with one "I" or with several? If we assume that it is correlated with several, how can we account for the fact that the complex experience is yet *unitary* and is simply an experience which "I" have? Are the manifold experiences of any one moment correlated with different substrata, although experientially and despite all manifoldness all of them belong to "my" "biography"? Or does the experiential unity of awareness imply one unitary substratum as the center of that "biography"?

Add to the manifoldness of experiences which "I" have *now*, at this particular moment, the great variety of "past" experiences of which "I" am aware, which "I" recall, and which are also constituent elements of "my" biography," and you will find that the "serial theory" of the self encounters still further difficulties; for no causal theory (in the Humean sense) can explain the experiential self-identity, the "unity of consciousness," which makes "me" recognize "my" own past. For one thing, the continuity of "my" experience is not interrupted. "I" sleep nights or, at times, "lose consciousness" altogether. But after such disruption of the chain of "my" experiences, "I" still retain "my" self-identity, and "my" "biography" continues as the unitary "biography" of *this*, and no other, "I." Whatever unconscious "mental events" "I" may assume as filling the gaps, they can be but "posits" and interpolations, implicates of experiential contents of which "I" am fully aware.

After all has been said and done, the continuity of "biography" as disclosed in "my" first-person experience, remains unintelligible without the assumption of a corresponding continuity of the substratum in which it is centered. No matter, therefore, from what angle we approach the problem, the insufficiencies of the "serial theory" suggest strongly that only a "substratum theory" can account for the nature of the self (cf. Gallie).

For the present it is sufficient to have made this point. How the substratum of the self is to be conceived and interpreted and how it is related to the "things" of the "external world," to "material substances" and their interactions—these are problems which may well

be left for later considerations, just as the nature of "things" must be considered at a later time. Some general remarks, however, may still be in order.

(a) As is clear from the discussion of categories, the reference to a substratum does not in itself imply "substance" or "stuff." Our repudiation of the "serial theory" does not commit us to a doctrine of "mind-stuff" or to a materialistic interpretation of the self.

(b) All "mental events," as elements in a specific "biography," are "private" to that "biography." That is to say, "my" experiences stand to "me" in a relation of intimacy which makes them strictly "mine." The color "I" see, the sound "I" hear, the emotion "I" feel, the thought "I" think can be experienced, directly and immediately, only by "me." "I" am aware of them. But "you" or "anybody else" can know only *about* them, indirectly and "externally," and only if "I" in some way inform "you" about them (cf. Montague).

(c) As a *self* "I" am involved in time in a double sense. (i) "I" experience a succession of events in the "specious present"; and (ii) "I" carry with "me" the past as an enduring "biography." "I" experience myself in both respects as an *enduring self*.

(d) As an enduring self "I" am also organizing, arranging, and integrating into coherent systems the various fragments of order which "I" find in "my" first-person experience; and ultimately "I" am aiming at one all-inclusive and perfectly integrated system. "My" scientific and philosophical activities have here their roots, and "my" practical living presupposes at least some measure of success in "my" process of integrating the objects of "my" experience.

(e) The integration "I" aim at necessitates an anticipation of the future; and as enduring self "I" transcend in "my" imagination the past and the present and posit "goals" which "I" try to realize. "I" work toward specific ends, and what "I" do is permeated with purpose. Hence, whatever else "I" may be, as a *self* "I" am at least enduring, integrating, and purposive.

Such, at any rate, is a reasonable interpolation of what "I" experience directly of "myself" in "my" own first-person experience. We can, however, go a step further.

My Body and the Pattern of "Things"

Among the things constituting the coherent and continuous pattern, and interwoven with them in the context of interdependence, there exists one "thing" which is of special significance to "me," namely,

my own *body*. This body occupies space just as any other thing, and it interacts with the things. It prevents other things from occupying the same space which it occupies at any given time; and as it presses against things they are pushed out of the places they occupy or are modified in shape or size. In turn, however, my body is also acted upon by the things and depends upon them for its proper functioning and its very existence. The "tissues" of my body are composed of the very same types of elements which constitute the rest of the bodied world, while the "food" I eat and the "air" I breathe are but parts of the pattern of things. There can be no doubt about it; my body is one of the things in the realm of things, and is interdependent with the whole pattern.

My body, however, stands also in a specific and unique relation to "me" *as the knower*. Only if *my* eyes are open do "I" see the things of the external world; only if *my* finger touches a hot stove do "I" feel the pain; and only if *my* body moves about do "I" observe the characteristic perspective changes in the realm of things. In brief, all contents of "my" experience which pertain to the pattern of things depend in some way on the presence and the functioning of *my* body. And when I touch a part of *my* body "I" can do so only through the employment of some other part of that same body. Destroy part of my body, and you encroach upon "my" experience, for you eliminate from "my" experience all those qualitative contents which depend upon that part for their occurrence. Anesthetize my whole body, and "my" experience as a whole ceases. My body, therefore, is an indispensable condition of "my" experience. "My" experience depends upon it and upon its functioning.

My body thus occupies a unique position. On the one hand, it is part of the pattern of things. In fact, it depends for its very existence upon that pattern and upon its own interaction with it. On the other hand, my body is an indispensable condition of "my" experience. "My" experience depends upon it.

"My" experience, however, is real. It *is*. It is the only occurrence of which "I" have irrefutable evidence. Any questioning, doubting, or denying of "my" own experience is but a further demonstration of its reality. Now, something real cannot possibly depend upon something non-real as its indispensable condition. My body, therefore, as an indispensable condition of "my" experience, must be at least as real as "my" experience itself; and the pattern of things, which is the indispensable condition for the existence of my body, must be as real as

my body. "I" can therefore affirm at long last that the pattern of things which I call "nature" or "world about me," in all its coherency and continuity, is as real as I am, and that it *is* in the same sense in which I assert reality of my body. The "trees" here and the "mountains" yonder are *as real as I am,* as real as *my bodied self.* A more profound reality I cannot attribute to anything.[7]

The objects of "my" dreams and hallucinations, however, do not share in this reality because they find no place in the pattern of things.

Other Persons and the World About Us

Among the "things" of the external world, and woven into their pattern of interdependence, are bodies like my own. They interact with the things very much in the way I do; and, from their movements, from their reactions and expressions, from their "words" and "deeds," "I" infer that they are embodiments of other focal points of experience, that they are living, sentient, thinking beings—such as "I" know myself to be.

Although in many of my contacts with them "I" know my "fellow men" only as centers of bodily behavior (just as "I" know "stones," "trees," and "birds"), occasions arise again and again when that behavior bespeaks the presence of an inner directing "agency" which "I" can conceive only in analogy to myself as a knower. On such occasions "I" can identify myself imaginatively with these "other persons." "I" can imagine their thoughts, their feelings, and their desires, and can thus comprehend them as minds.

Such "inference by analogy," however, is not the only, nor even the most important, way in which "I" know my fellow men as centers of first-person experience. My contact with them provides much more direct and much more convincing evidence; for "I" can *communicate* with my fellow men. We exchange ideas. We understand one another. We raise and answer questions and discuss problems of various kinds. And such communication, as we have seen earlier, is possible only upon the presupposition of a "mutuality of minds." In and through the dialectic of communication and discussion the "other person" stands revealed as being what "I" am—an enduring, integrating, and purposive center of first-person experience.

Perhaps all this is faith—but if so, it is faith not in "my" own

[7] Fundamentally this seems to be also the thesis of the "existential" philosophy of Heidegger and Jaspers. Cf. W. H. Werkmeister, "An Introduction to Heidegger's 'Existential Philosophy,'" *Philosophy and Phenomenological Research,* II, 1941, 79-87.

whims and wishes, but in the basic orderliness of the world, and in the coherency and continuity of the pattern which constitutes that world. It is no longer mere "animal faith" but a reasoned and warranted belief which finds unquestionable verification in our daily living.

If my fellow men were mere delusions of "my" mind, and if they were no more real than are the objects of dreams, much of the pattern of the world would be disrupted and its coherency shattered. There would then be no accounting for the existence of houses and cities and motorcars, and no explanation for books and pictures and musical compositions. All "human productions" would have to be eliminated from the world of things, and the pattern of that world would be incomplete.

The attack upon the thesis that "other minds" exist follows two distinct lines of argument. (1) The behaviorist is forced to deny their existence because he denies, in principle, the existence of mind altogether—including that of his own mind. But the radicalism of this position impairs the argument itself; for, in conformity with his theory, the behaviorist must regard himself as nothing but a system of bodily behavior-patterns and, as such a system, he cannot cognize or theorize about anything. Cognition and theorizing presuppose "mind" at least in the form of symbol-consciousness; for meaning itself is impossible without this. But if the behaviorist must admit that at least he himself is a knower, a "mind" in the sense of symbol-consciousness, then the principle upon which his argument against "other minds" rests breaks down and his argument itself becomes inconclusive.

(2) The solipsist also denies the reality of "other minds"; but he asserts at least the existence of his own mind. His argument derives all its force from the so-called "egocentric predicament," i.e., from the fact that the experience of any given knower is in a very specific sense "private" to that knower and therefore inaccessible to anyone else. "Other minds," so the argument now runs, do not and cannnot exist in the world of "my" experience because they (and their experience) are "inaccessible" to "me." The force of this "argument," however, is more apparent than real; for there is nothing in the nature of a given experiential content which would prevent this content from becoming part of anybody's experience. That "I" am aware of it rather than "you" (or vice versa) is due to circumstances which are contingent to the nature of the content as such and cannot be inferred from the content itself. If "I" see a particular *red* or hear a particular *sound,* there is nothing in the quale of the *red* or the quale of the *sound* which

would preclude the possibility of its ever being seen or heard by somebody else. But if this is so, then the "inaccessibility" or "privacy" of first-person experience—genuine though it be in one sense—is not such that it eliminates in principle the hypothesis of the existence of "other minds." The solipsist's argument, therefore, does not impair our thesis.

But if "other persons" exist as experiencing, thinking minds, then their reality casts new light upon "my" conception of things "out there"; for now "many" persons may have parallel experiences of the "same" things. Our experiences may overlap in a determinable way and may augment one another.

If "I" see a "book" and locate it as "here" by touching it with my hand, "you" may also see a "book" which you locate tactually in the same place in which I locate the book "I" see; and, from the description of what we see and feel, we discover the interrelation of our experiences—their basic parallelism. A thousand persons may "see" the "same" thing or event, and millions may observe the "same" solar eclipse, and the mutual corroboration of their respective experiences will be a welcome supplement to "my" criteria of the things which constitute the world about us. When "my" own observations are interrupted by sleep or death or in some other way, "other persons" can carry on and can see the pattern of the world in uninterrupted continuity.

In order to account for this parallelism of experience we must either resort to the doctrine of a "pre-established harmony" according to which all human beings were "predetermined" in their inner constitution to have "parallel" experiences at the "proper" times; or we must admit that the things "out there" really are *out there*—that they transcend "my" consciousness and "yours" and that of every other finite being, and that they have a reality and an existence of their own. The former alternative not only taxes our credulity to the limit but can be shown to lead to various contradictions. The latter alternative is, by comparison, simple and "natural" and free from contradiction. It is really the only one which is reconcilable with the thesis that the world about us is the indispensable condition for the existence of an indispensable condition (our body) of our own first-person experience.

Interpreting the Pattern

The thesis that the things which constitute the pattern of the world have an existence which transcends any particular first-person expe-

rience does not imply that the things or their patterns can be described or understood in terms other than those of human experience. When the astronomer predicts a lunar eclipse, he means that if we observe the "moon" at the specified time, we shall have all the visual experiences characteristic of a "lunar eclipse," and that if it were possible for us to observe the predicted event from some point outside our solar system, we would find three numerically distinct bodies, the "sun," the "earth," and the "moon," in specific spatial relations to one another. This "constellation" of celestial bodies would, of course, occur even though nobody were here to observe it, but we can speak about it and can understand it only in terms which derive their meaning from human experience. How else would we know what it means to say, "three bodies in a specific constellation"?

When, on the basis of "historical data," we reconstruct past events —such as Caesar crossing the Rubicon or the formation of the Rocky Mountains—then this reconstruction, too, must be given in terms of human experience. What we mean to say is that if we had lived during the time of Caesar we could have observed his troops in the process of crossing the river; or, if we had lived during the proper geological age, we could have observed the upheaval of the American continent which resulted in the formation of the Rockies. Our interpretation of present experiences, or experiential contents here and now before us, may lead to a description and interpretation of things and events far away in space and in time, but we understand those things and events only in terms of possible human experience. "Things" and "events" which cannot be so understood or which cannot be connected, indirectly at least, with our experience, cannot be understood at all, and reference to them is meaningless. First-person experience and the integration of that experience into a coherent and continuous pattern of interdependency are our only cues to reality and our only means of its comprehension. They are the sole basis of science and philosophy.

But let us approach the matter from a different angle.

Categories of the External World

In an earlier section of this chapter we have discussed briefly the categories which permeate the whole of first-person experience and contribute to its order. It is evident, however, that these, the most general, categories are not sufficient for an interpretation of the world about us. The order of that outer world depends upon additional categories, i.e., it depends upon categories which are consistent with, and related to, the categories already discussed, but which, neverthe-

less, involve aspects of experience not covered by the earlier table. Representative of these additional categories are the following pairs:

Harmony—Incompatibility
Inner—Outer
Determination—Dependence
Element—Structure

As before, each pair of these categories constitutes a categorical complex; and the four pairs together form a system of closely interrelated categories. The meaning of any one of these categories, therefore, entails them all (cf. Hartmann, 1940).

(*a*) *Harmony—Incompatibility.*—The meaning of these two categories must not be confused with "consistency" and "contradiction"; for the latter concepts pertain to thought and the logical relation of ideas, whereas the categories here under consideration pertain to the world of things and to the interrelation of things. "Contradiction," to be sure, is also a form of "incompatibility," but it is an incompatibility of ideas, not of things. And "consistency" is a form of harmony; but it is not a harmony or compatibility of things.

Incompatibility, in the realm of things, means an opposition of "forces"; it means repulsion, conflict, struggle, the interference of one thing with another. And harmony means that despite this conflict and struggle the world of things does not burst asunder but endures. Individual things may be destroyed in the clash of conflicting forces, but the world as a whole remains. Actually, the incompatibility of things and forces, and the conflict and struggle which this involves, constitute the dynamic aspects of a world which, in a deeper sense, is yet one. The very oneness of that world bespeaks a fundamental harmony which transcends all conflict. Were it otherwise, the "world" of things would be chaotic and would not be a world at all.

If a thing, A, occupies a certain space, another thing, B, cannot occupy the same space at the same time. One thing thus "repels" another. But every action produces an equal and opposite reaction, and actions and forces are balanced in dynamic equilibria. Any shift in the balance reveals the conflict of forces. It "liberates" energies and produces changes until a new balance, a new harmony has been achieved. Whether we examine the physical realm proper or the sphere of human relations, the interdependence and the dynamism of incompatibility and harmony are clearly discernible. They are that which makes the world of things a "going concern."

(*b*) *Inner—Outer.*—In a sense it is obvious that any three-dimen-

sional thing has an "outside" and an "inside," a "surface" and an "inner core." But in this sense the relation of "inner-outer" is of little significance; for the "inner core" is a vague and obscure "something" which always escapes us as we try to observe it. We can see and touch "outsides" and "surfaces" only.

A more significant aspect of the relation of "inner-outer" will be encountered when we raise the question of the "external" world as such. In what sense is that world really "external"? An obvious answer is, of course, that it is "external" in the sense of "being outside my body." But this can hardly be the whole story; for "my" body itself is part of the "external" world and is "external" to "me." "My" experiences, although dependent upon my body, constitute an "inner core" of "mental events" of which the body is not a constituent part. On the other hand, however, "my" actions in the world about me can be effected only through my body; and in this sense my body is the outward manifestation of "my" will. "I" decide to pick up a book; my arm reaches out; my hand takes hold of the book; my body executes "my" decision. Here, then, is an "inner core"—"my" will—which "I" experience, as it were, from the "inside" in a dynamic rather than a spatial sense, but which becomes effective in an "outer" form through a bodily manifestation in space. And it is in this sense, I believe, that "outer" and "inner" are of categorical significance in the world about us.

In the physical world, the "inner core" of a thing may be seen in the dynamic equilibrium of forces, of opposed stresses and strains, of balanced actions and reactions; while the "outer" manifestation is the relatively stable constellation in space. Atoms and molecules have an "inner" and "outer" in this sense, and so have planetary systems and galaxies.

At the level of plant and animal life the inseparable interrelation of a dynamic "inner core," in the sense of opposing yet partially balanced "forces," and an "outward" manifestation in space of the bodied whole is perhaps even more strikingly evident than it is at the level of inorganic things. And still more apparent is it where human beings, "persons," are involved. The whole complex of feelings, urges, desires, instincts, and volitions constitutes the dynamic "inner core" of our "outer" body's existence.

The aspects of "inner" and "outer" are, finally, also encountered at the social level. They may be discerned in connection with "institutions," "social groups," "society as a whole," or whole "cultural

eras." Wherever there is a "going concern" of social living, there we also find the "inner" dynamics of opposing forces, and the "outer" manifestations of changing balances and equilibria. The categories "outer-inner," therefore, permeate the whole "external" world and all of its "things."

(*c*) *Determination—Dependence.*—Our criterion of the external world has been, and is, the coherency and continuity of the pattern, and the interdependence of things. The categorical complexes "harmony-incompatibility" and "outer-inner" describe specific phases of the patterned interdependence but do not adequately express the element of "necessity," which is characteristic of the interrelation of things. This "necessity," on the other hand, does find recognition in the mutually complementary categories "determination" and "dependence."

As here understood, i.e., as categories of the external world, determination and dependence do not designate the logical relation of implication but refer rather to the various factual relations of things as things. They do designate relations, however; and primarily relations of the "sequential" type. The bipolarity of the relation indicates that we are here dealing with a specific dimension of order which is constitutive in the world about us.

The categorical complex "determination-dependence" implies that the things of the external world do not exist in supreme independence of one another, that they do not merely co-exist in space or succeed one another in time, but that one thing exists *because of* another and through the efficacy of that other. The relation, in other words, is of a dynamic character. One relatum is the "determining" factor, the other the "dependent" one; and the two belong together in a sequence of existing things. "Determination" and "dependence" are only the discernible poles of this unitary relation as viewed from opposite directions.

It is relatively simple to discover in any given situation which is the dependent thing. It is rather difficult, however, in most cases to specify the determining relatum; for, as a rule, the determining factors are complex and varied. What, for example, is the determining factor when in the presence of soil, moisture, and sunshine an "acorn" grows into an "oak tree"? That the "oak tree" is somehow dependent upon all the factors mentioned seems certain. It is in every respect a dependent being. But the more we succeed in analyzing the factors upon which the "oak tree" depends (for its nature as well as for its

existence), the more we are impressed by the complexity of these factors and by the intricacy of their interrelations.

The simplest form of the "determination-dependence" relation has commonly been called *causality*. It is exemplified by the "push" type of influence which one thing exerts upon another. In this sense, "determination" and "dependence" are closely interlinked with the spatial incompatibility (or "impenetrability") of things, and form the basis of all strictly "mechanistic" relations involving the passage of time.

Of a somewhat different type is the "determination-dependence" relation encountered in all forms of interactions in which the time element is irrelevant. Gravitational attraction exemplifies this type; but it is also discernible as the "binding force" in molecules and as the "inner core" of dynamic equilibria.

In the realm of organisms (both plant and animal), the individual chains of "determination-dependence" are, furthermore, integrated in such a way as to constitute "teleological" complexities in which the "whole" determines the function and the fate of each "part." The character of this "organismic" relation may well be regarded as constituting an additional type of determination.

Finally, at the human level, the "organismic" type is supplemented by a "determination-dependence" relation which involves "purpose" and a conscious striving after goals.

Each and every one of these types of the "determination-dependence" relation involves its own problems and provides its own difficulties of analysis. But such detailed matters cannot be discussed at this time. Nor can we settle here the question whether or not all of these types are ultimately reducible to some one type; and if so, what that type is. It suffices at this time to have separated the various types from one another and to have identified them as constitutive elements in the pattern which is the external world.

We said a moment ago that the "determination-dependence" relation implies an element of "necessity." This statement must not be misunderstood. Any attempt (such as the Humean) to derive this "necessity" empirically must come to naught; for the "necessity" here referred to is of a categorical nature and therefore non-derivable. It is an ***indispensable presupposition*** for our understanding of that pattern of things which constitutes the world about us. After all, we have knowledge of an external world only because of the "necessity" involved in the "determination-dependence" relation, and because some of the objects of our first-person experience can be subsumed under this

"necessity"; but we do not derive, by empiricist abstraction, the idea of necessity from a world of things antecedently known to us. The Kantian argument is here irrefutable.

(*d*) *Element—Structure.*—The things of the external world, as semi-permanent and three-dimensional entities, have "structure" and consist of "elements." The interrelation of "structure" and "elements" is one of mutual determination-dependence. The nature and function of the "elements" is determined by the position which the "elements" occupy in the "structure," while the "structure" is what it is because of the "elements" which constitute it. That is to say, "structure" and "element" occur only in indissoluble fusion. There exists no "structure" without constituent "elements," and there exist no "elements" without functional relations of some sort to a "structure."

The "structure," nevertheless, has a certain independence of the "elements"; for, within limits, different "elements" *of the same kind* may occupy the same position in any given "structure." But the "elements" are correspondingly independent; for the same "element" may at different times be included in different "structures."

The "elements" themselves may be "structures" of a "lower" type; and a given "structure" may, in turn, become an "element" in a "higher" "structure." Electrons, positrons, and neutrons are thus "elements" in the "structure" atom; but atoms are "elements" in the "structure" molecule; molecules are "elements" in the "structure" organic tissue; and organic tissues are "elements" in the "structure" organism. Some "structures"—such as houses, bridges, snowflakes, and solid crystals of any kind—are essentially *static*; while others—such as plants and animals—are fundamentally dynamic and depend for their very existence upon a development and constant adjustment of their structural "elements." They are "structures" *in process.*

Obviously, the categories "structure" and "element" possess some of the characteristics which we noted in connection with "form" and "matter"; yet these two pairs of categories are not identical. The categories "form" and "matter" pertain to the whole of first-person experience and are valid for dream objects and the imaginings of our hallucinations no less than for the experiential complexes which are things in an external world. The categories "structure" and "elements," however, are valid only for the latter, i.e., they are valid only for the three-dimensional entities in space; and they imply a *dynamic interdependence* of "part" and "whole" which is not present in the interrelation of "form" and "matter."

In a sense, of course, "structure" and "element" are but special cases of "form" and "matter"; but they are "form" and "matter" permeated with "harmony" and "incompatibility," with "inner" and "outer," and, most important, with "determination" and "dependence." They are categories of the external world, not simply categories of the contents of first-person experience.

One other point must be noted. What I call here "structure" and "element" have often been referred to as "whole" and "part." I have no objection to this latter terminology so long as the "whole" meant is a "*structural* whole" and so long as the "parts" are "*structural* parts" *capable of mutual interaction.* But when the "whole" is conceived as a mere aggregate and when the "parts" are but unitary and mutually indifferent "additives," the confusion of "whole" with "structure" and of "part" with "element" is inexcusable. A "whole" may be the sum of its "parts"; but a "structure" is never a mere sum of its "elements." The interaction and dynamic interdependence of the "elements" make it something entirely different—make it a "structure."

Purpose and Teleology

In connection with "inner" and "outer" we pointed out that the most significant meaning of these two categories stems from the fact that we experience ourselves in a double rôle, that, namely, of an "inner" center of "mental events," and that of a bodied existent in the world of things. The fact of "self-expression" provides, however, a clue to the understanding of other categories as well, and notably to that of the categorical complex "determination-dependence."

David Hume, it will be remembered, repudiated the causal nexus of things because, as he said, "the mind never perceives any real connexion among distinct existences." However true this Humean statement may be in the realm of things—and in the empiricist sense it is true—it is manifestly not true in the mental sphere; for if we are annoyed or pleased, we are annoyed *by something*, pleased *with something*; and the "something" is usually a *specific* "something." That is to say, we experience the "cause" with the "effect." When I am glad to see you, "I" experience the "gladness" as the direct result of "seeing you"; i.e., "I" experience not only "gladness" and "seeing you," but a relation of "determination-dependence" which connects them. Similarly, if you make a remark which angers me, "I" do not merely experience your "remark" and my "anger" but also the inter-

connection which makes my anger an "*anger-because-of-your-remark.*"

In the realm of things our knowledge of causal connections may never be completely free from elements of hypothesis; in some of our psychological reactions, however, the interdependence of "mental events" constitutes the very core of the experiential complex and is known to us directly and indubitably.

We are aware of the fact that at least some of our actions are really "reactions-to-stimuli."

The categorical complex "determination-dependence" is experienced with even greater clarity (but not necessarily with more certainty) whenever we try to reach a specific goal or when we employ "means" toward an end. When "I" decide to take a walk, "my" body responds and carries out "my" decision. The movements of my legs are determined by "my" resolve. They continue or stop (normally) as "I" determine. This does not mean that "I" am aware of all phases involved in the transition from conscious decision to bodily response; but it does mean that "I" feel that the motion of my body somehow depends upon "my" decision. In the control which "I" have over my body, "I" experience immediately and directly the "determination-dependence" relation which cannot be so directly observed in the realm of things.

But this is not all.

All voluntary actions serve some "end." They are carried out either in conformity with, or in opposition to, certain "tendencies" or "inclinations" which "I" have, and they tend to alleviate particular "needs" or to satisfy some "interests" which "I" experience. To "will" something, in other words, is equivalent to "having a purpose," to "trying to reach a goal"; and the *intention* of a goal is immediately given in the characteristic experience of a voluntary act.

Intending a goal and achieving it are, however, two entirely different matters. Frequently, the achievement of a goal depends upon the employment of proper "means." Such means must be selected and, at times, produced before they can be employed. And in this process of selection and production of the "means" to an end we encounter once more the "determination-dependence" relation as an experiential reality; for if the goal has been agreed upon, we have only a limited choice of means for its realization and we experience this limitation, this hindrance to unrestricted freedom of action, as a form of "determination-dependence" which we cannot escape. We experience it as a compelling necessity; for our will remains frustrated and impotent as

long as we fail to employ the means necessary for the realization of its goal.

In the strict sense, all purposive action involves a "being aware" of the goal or a "being aware" at least of the general direction in which the goal may be found. Purposive action, in the full sense, is therefore largely restricted to the levels of intelligent behavior. We know it directly and as a fact only in human experience. We may assume that some of the higher animals also are capable of it; for at times their behavior is strikingly "human." But no amount of "external" observation of animal behavior can provide the proof which, at the human level, comes from our own experience.

However, we must supplement these considerations by an analysis of something more basic in our experience than voluntary actions. I refer to our "instincts" and "drives," to the primary "urges" of hunger, thirst, and sex, as they enter into our own self-experience. In and through these "urges" we experience ourselves as centers of restlessness and "tension," as "keyed up" for action in certain directions. And these directions imply certain goals. They imply, broadly speaking, the preservation of the individual and the perpetuation of the species.

I am not saying, of course, that these goals are consciously present in our "urges," although they may be; I am merely indicating them as objectively discernible consequences of our "drives." The direction toward a goal is there although the goal itself need not be consciously fixed or deliberately agreed upon. Action impelled by our "urges" is not the same as voluntary action. And if the latter is regarded as *purposive*, the former may be called *teleological*; for it is directed toward a goal; but it is so directed "blindly" and not as the result of deliberation on our part.

We can, of course, choose (within limits) *what* we are going to eat or *what* we are going to drink, and we can (again within limits) restrain our sex urge or direct it into channels of our own choosing; but these matters are irrelevant to the fact that, *inherently*, every one of our instinctive urges is directed toward a goal and that it is therefore teleological.

Moreover, our basic urges find satisfaction through the employment of certain parts of our own body as "means" toward an end. These body-parts bear the imprint of the end to be achieved. The incisors and molars, for example, and their positions in the jaws, the taste-buds of the tongue, the esophagus, the stomach, the whole system of

glands and internal secretions, of balances and adjustments of the "internal environment"—all this is intelligible only as means to an end. The teleology of functions is here reflected in the structure in the same way in which the teleology of functions is reflected in the structure of man-made machines which definitely embody a purpose.

At the level of the higher animals we encounter comparable structures and, in all essentials, a corresponding type of behavior. Can we say, therefore, that these animals are centers of "urges" and "drives" such as we experience ourselves to be? And if we can say this concerning the "higher" animals, can we say it concerning *all* animals, including the unicellular creatures which multiply by cell-division? Does our argument from analogy, supplemented as it is by a study of organic structure, still carry conviction? Can we ascribe to a plant something comparable to the "tensions" which our "urges" produce in us?

We are here on dangerous ground and it is too easy for comfort to fall victims to our own rationalizations. Human experience provides a key to the understanding of nature, to be sure; for human beings are part of the pattern that is nature. But this does not justify reading human experiences into everything that is happening in nature. On the contrary, the overwhelming success of modern science has been achieved largely by going counter to all anthropomorphic tendencies and by eliminating more and more the "human equation" from all fields of study. Whether this tendency of modern science is the final word in the matter or whether it will in the end lead to insurmountable difficulties remains as yet to be seen. In a sense, the remainder of this book is devoted to an attempt at clarification of this point. But our task is arduous and our road long, and many are the opportunities for getting lost in the wilderness of preconception and theoretical bias. Only success or failure in the interpretation of the *whole* of first-person experience can decide the issue.

CHAPTER IV

TRUTH

Different Interpretations of the World

The pattern of the world about us, referred to in the preceding chapter, is as yet undefined in detail and ambiguous in general character. We have, of course, discussed various categories which determine its structure and stratification, but the section on "Purpose and Teleology" has revealed that so far our analysis has led to no unequivocal interpretation of the whole. We only know that whatever knowledge of the external world we possess must have its roots in our own first-person experience and that we can know nothing of that world except through the integration of the elements of order discernible in our experience. But the pattern of that order may be conceived in different ways. The "mystical" pattern of primitive man, for example, is quite different from the "mechanical" pattern of nineteenth-century science, and both differ in fundamental respects from the patterns defined by contemporary science and by Christian theology.

From the point of view of critical philosophy, the mystical conception of the world is not just a first step toward our "modern" view, but is (like scientific cognition, art, and morality) an independent and in itself complete integration of experience—an integration, which cannot be evaluated properly in terms of extraneous standards of reality but which can be understood, if at all, only in terms of its own inner structure. It presupposes its own values, its own modes of "necessity" and, therefore, its own criteria of reality. It is a specific manner of "picturing" the world, of integrating first-person experience; a unique point of view from which the whole range of experiences is seen and molded into a distinctive pattern of things.

The "world" of primitive man is as much a "construction" as is the "world" of the scientist, but not more so. It is the result of an integration of experience and is determined throughout by the specific acts of objectification which transform the contents of first-person experience into definite objects, and which weave these objects into a pattern of things. The characteristics which distinguish the

world of primitive man from the world of the modern scientist are direct results of the differences in the integrative processes and stem from differences in the conception of the anticipated pattern.

Such differences in interpretation would be impossible if things were "given," directly and immediately, and were not integrative complexes of first-person experience; i.e., if prior to every intellectual comprehension, prior to all interpolation, the content of first-person experience already implied a necessary and inescapable classification of objects. We have seen, however, that things are themselves but objectifications of experiential complexes and that the whole process of objectification enters into their constitution.

The existence of space, for example, is as such not "given" with the contents of experience. The three-dimensionality of things, their definite position in three-dimensional space, and their definite distances from other things—all this is not "given," immediately and directly, in first-person experience but is a matter of integration and of constructive interpolation, and the whole conception of "physical" space is the product of a constructive imagination. It is a systemic context. And to the extent to which this is true, different contexts may be constructed and different "spaces" and, therefore, different "worlds" may be conceived.

The transition from distinct and disparate contents of first-person experience to the pattern of things is possible only because in the flux of our experience certain contents and relations recur and can be identified. They constitute the first elements or fragments of order, and form nuclei around which, in time, we organize the whole of experience.

It is important to note, however, that the "given" contents of experience, as soon as they are discerned as distinctive, are seen also in relation to one another and in correlation with the whole of experience; and they must "prove" themselves in this correlation. Not only do we comprehend them intellectually as constituent elements in a context, but even in their "given" nature they reflect the correlation with other contents of experience. A specific shade of green, for example, will "look different" in different combinations with other colors; and the *Praegnanz* (or lack of it) of a configuration determines unmistakably the experiential qualities of the elements entering into that configuration. Not one content of experience is ever "given" all by itself and without some relation to other contents. The isolated "datum" does not exist. It is a "boundary

concept," the result of an abstraction. Even the most primitive perceptual contents are inseparable from a selective emphasis of "essential" and "non-essential" elements.

The possibility of this "selective emphasis" is one of the conditions which enable us to subsume different contents of experience under one concept and to integrate them into a definite and determinate object. The process of integration, therefore, permeates and transmutes the whole of experience, and any difference or shift in the "selective emphasis" must be reflected in the result of the integration.

To what extent the integration of experience as carried out by primitive man differs from the integration accomplished by modern science may readily be seen from the respective attitudes toward dreams and hallucinations. It may be discerned also in the demarcation or lack of demarcation between the living and the non-living, and in various "practices" which concern the world about us. We combat diseases through drugs and vaccines; primitive man prays to his gods and relies upon rites and incantations. We take it for granted that the "laws of nature" govern thunderstorms and tornadoes; primitive man sees in these phenomena an expression of the wrath of supernatural beings, and acts accordingly.

The very meaning and function of "concepts" is different from the two points of view; for the "collective representations" of primitive man are not really "concepts," as we understand that term. They do not possess the same purely representative character as do our concepts, and they are not subject to the same rules of logic. Primitive man, as a rule, thinks in terms of images, and he does not differentiate clearly between the logical content of his images and the hopes and fears which the images call forth in him. No object or thing, therefore, is for him what it appears to be to us. The elements of experience which we regard as important, primitive man may completely disregard; while he, in his turn, may stress matters which escape our attention or which, to us, are unrealities. For him, each particular thing has "affinities" and "powers" which we do not recognize or acknowledge, while our conception of causality is meaningless in his world-view.

As far as primitive man is concerned, various parts of the human body—the heart, the liver, the kidneys, the fat, the marrow, the hair, the blood, and even the excreta—possess "mystic" powers and exert "magic" influence. The significance of these parts transcends all biological functions proper, and their "magic" is more important than

is their causal efficacy. Even the things made by man and used by him as tools or weapons possess their "magic" and are useful and potent only because of their special "powers." The whole social and physical environment is, for primitive man, permeated with mystic "affinities"; and in harmony with these "affinities" he constructs and integrates his world.

Primitive man does not merely *associate* mystic powers with the various things; he regards those powers as of the *very essence* of the things. "Thing" and "powers" still constitute an undifferentiated whole. The distinction between them, the intellectual separation of the thing and its attributes, marks an advanced stage in cultural development and a degree of reflective abstraction not found at the level of "pre-logical" thought.

Since primitive man knows no phenomena apart from magic influences, i.e., since he knows no "natural" phenomena in the sense of modern science, our scientific "explanations" are meaningless to him, and our criteria of objectivity and reality prove nothing as far as he is concerned. He freely "communicates" with "spirits" and "souls" and with "totem animals"; and the objects of his dreams are as real to him as are the things of the "external" world. Actually, his contact with things is itself a "communication" with intangible and invisible "powers," with "spirits" and "souls" which, for better or worse, affect the welfare of man and determine his fate.

If this mystic conception implies that certain things possess specific "magic" qualities, no amount of "scientific" reasoning will convince primitive man that the "qualities" do not exist. The fact that they cannot be observed or that they are not effective when put to a test proves nothing; for these qualities, by their very nature, may be "invisible," and doubt and skepticism may make them ineffective. Experimental tests, therefore, decide nothing. Primitive man requires no "proof" or "experimental verification." His belief in the magic property of things is independent of all "demonstrations" and cannot be shaken by the lack of "testable evidence."

The world-view of primitive man is, nevertheless, the result of an integration of experience—just as our world-view is such a result; but it is the result of an integration which is dominated by what Lévy-Bruhl has called *"the law of participation,"* not by the logical principle of identity or the scientific law of causal relations.

This does not mean that primitive man is unaware of the specific shapes or colors of individual things, or that he fails to distinguish one

thing from another; but it does mean that things do not exist for him *as bare facts*, that they play a part in his personal or tribal life primarily because of the magic powers they possess and because of the penumbra of mystic affinities which surrounds them. Nature, to primitive man, is a realm of invisible and mystic affinities sustained and controlled only by magic powers and by forces subject to magic controls. Sickness and disease are the results of "spells" and "curses" and other mystic influences. Every startling or unusual phenomenon is a "sign" of some impending event; but as a "sign" it may also be the very cause of the expected event. The rising of the sun and the "growing" of the moon, rain and fair weather, the regularity of the seasons, the productivity of the field, the abundance of the harvest and of animals to hunt—these and many other "events" must be secured through the magic power of rites and incantations and various sacrifices. And the future can be assured only if "magic controls" ward off "evil influences" and secure the co-operation of "friendly spirits."

The law of mystic "participation" allows no exception. Its efficacy permeates the whole of experience and its synthetic and integrating power determines all. The world-view of primitive man is thus self-sufficient and in itself complete. It is, in principle, as universal in scope and as all-inclusive as is the world-view of science or the world-view of Christian theology; only it is a world-view of a different type, constructed with a different pattern imposed upon the contents of first-person experience.

Which is the true interpretation of that experience?

We point, of course, with pride to the accomplishments of modern science, convinced that *there* is the truth. But the question cannot be answered so easily as that. The philosopher, by the very nature of his task, is compelled to justify his choice and to give reasons for his answer. And are we sure that modern science gives us the *whole* truth; that there is no truth outside the sciences, and that all non-scientific interpretations of experience are but aberrations and deviations from the truth? After all, what *is* truth? How do we recognize it? How can we come to know it? And is it one or many? The problem is not as simple as it appears to be at first glance.

Ambiguity of the Concept 'Truth'

The word 'true' ('truth') is derived from the Anglo-Saxon word "treowe," meaning *faithful* or *trusty*. One may therefore speak of a

true friend, meaning a faithful or loyal friend; or one may designate, in general, anything as *true* if it can be relied upon.

But the word 'true' may also mean *genuine*, as in the statement, "This crystallized carbon is a true diamond"; or it may mean conformable to a standard, as in the assertion, "This picture is a true likeness of the deceased"; or it may designate conformity to type, as in the statement, "Mutants breed true." All of these meanings—and several others might be added—are accepted as "good English," but they are not what the word 'true' connotes in epistemology.

According to the dictionary, 'true' means also *conformable to fact, correct, not erroneous*; and it is this sense of the word, and this sense alone, which concerns us here.

It is obvious from the epistemological meaning of 'truth,' as given in the dictionary, that this word is an adjectival noun; that it designates a quality—*conformable to fact, not erroneous*—rather than a thing. Moreover, the quality designated by the word can be attributed only to propositions. If this is not itself obvious, it is at least tradition in philosophical discourse. There exists no entity "truth," and certainly not an entity TRUTH (with capital letters). Only *propositions are true*. And to assert that a given proposition *is* true is to assert that it *conforms to fact* or that it is *not erroneous*.

So far, of course, we have only introduced synonyms of the word 'true.' We have indicated its different meanings and have selected one of these meanings as alone relevant to the problems of epistemology. We must now probe deeper into the meaning of 'truth' by asking what is meant by 'conformable to fact' and by 'not erroneous,' and how we can know whether or not a given proposition *is* "conformable to fact" and *is* "not erroneous."

Repudiation of Skepticism

Before an adequate answer to these questions has been given, the skeptic may deny the significance of our problem and may assert that there is no truth; that the search after truth is meaningless as well as futile.

The answer to the skeptic is quite obvious; for his denial of all truth involves him in an inescapable contradiction. Whosoever maintains that there is no truth either says nothing or he asserts a proposition which he himself must regard as true. And once it is admitted that at least one proposition is true, there is no a priori reason why

others might not also be true. The attainment of truth is then no longer impossible in principle, nor is the search after truth meaningless.

There is no escape from this conclusion even if the skeptic were now to assert that there is only *one* true proposition, the proposition, namely, that there is no truth. Such an assertion, after all, presupposes that we know something concerning the nature of propositions; that we know, for instance, how one proposition differs from all others. And it presupposes also that this knowledge is *true*.

Actually, the assertion that only the proposition denying all truth is true assumes true knowledge of such scope and detail that it transcends by far anything we can legitimately claim to know; for it presupposes that we know not only all possible propositions (past, present, and future), but also that *all but one* of these propositions are *false*—a presupposition which is impossible of demonstration (cf. Blanshard).

The denial of the truth of all but one proposition presupposes, furthermore, that we are in possession of a criterion of truth and falsity, and that this criterion is unfailing and can be relied upon in our dealings with *all* propositions.

In brief, the skeptic's denial of truth presupposes a knowledge of reality which far exceeds anything we have actually achieved in science or in philosophy; and it implies that this all-inclusive knowledge is true. We shall therefore leave the skeptic to his own designs and his own contradictions and shall turn to a constructive interpretation of the meaning and nature of truth.

"Self-Evident" Truths

Some propositions are said to be *self-evident* and therefore *true* beyond the shadow of a doubt (cf. Ducasse). The following statements are typical examples of the types of propositions included in this class:

A is not non-A.
Things equal to the same thing are equal to each other.
Two straight lines cannot enclose a space.
If A precedes B, then A precedes everything which is contemporaneous with B.
The red I see here differs in quality from the green I see there.
I exist.
It is my duty to produce the greater good rather than the less.

The great variety of propositions thus regarded as self-evident is surprising; for included in the list are not only principles of logic and mathematics, but propositions concerning experiential qualities and moral laws as well.[1]

However, many propositions formerly accepted as self-evident have been shown by philosophers and scientists to be false. The Aristotelian "law" of falling bodies and the contentions that the earth is at the center of the universe, that it is stationary, and that it is not round, are such disproved "self-evident" "truths."

If the criterion of self-evidence cannot be depended upon in *all* cases, what assurance do we have that it is trustworthy in some? And what, exactly, is the criterion of "self-evidence"?

Difficulties arise as soon as we ask what, precisely, distinguishes a "self-evident" proposition from other propositions; for we discover that there is no clean-cut line of demarcation. The difference is at best one of degree only. The whole history of the discovery of "self-evident" truths bears this out. Practically all of the so-called "self-evident" truths were discovered in the course of arduous and painstaking analyses and did not leap at once into prominence as something strikingly unique and inescapable. Often it was necessary to prove in a roundabout way the presumed "self-evidence" of a proposition. This is true, for example, in the case of the basic laws of logic. The proof depends here on showing that the laws in question cannot be consistently denied so long as we accept as valid any logical implications of this denial. If we deny, for instance, the validity of the law of contradiction, we must assert that the law itself is false rather than true. But if we do this, we actually assume the very validity which we intend to deny; for we assume that if the law is false it cannot also be true; we assume, in other words, that A is not non-A. The attempted denial of the law of contradiction thus entails an inescapable contradiction (cf. Blanshard).

But is this contradiction which is implicit in its denial proof of the "self-evidence" of the law as such? It is my contention that it is not; for I believe that "self-evidence" is one thing, and that the acceptance of a proposition as true because of what its denial entails is quite another. I would contend, in other words, that the fundamen-

[1] Cf. Blanshard, II, 238-249. The general position taken in this chapter with respect to the interpretation of truth is, to my mind, a necessary consequence of the views developed in Chapter III. The statement of this position is, however, strongly influenced by Blanshard's arguments. My indebtedness to Professor Blanshard is obvious to all who have read his magnificent work.

tal laws of thought must be accepted as true not because they are in themselves self-evident, but because without them no inference is possible and all thinking must come to a stop.

This applies to the law of excluded middle no less than to the laws of contradiction and identity; for if we deny the law of excluded middle—the law, namely, that X is either A or non-A—our thought is confronted with "objects" which are neither A nor non-A, or which are both A and non-A; and in either case intelligible thought is impossible—as may be discovered by actual trial.[2]

Turning to the field of mathematics, we discover that the "axioms" which provide the broad foundations of mathematical systems, are no longer regarded as self-evident truths. The development of non-Euclidean geometries played havoc with the earlier contentions, and mathematicians now speak of "postulates" and "primitive propositions" where their predecessors accepted "self-evident truths." Even so simple a statement as that "a straight line is the shortest distance between two points," is no longer regarded as self-evident; for its truth depends on the geometrical system within which 'straight line,' 'point,' 'shortest distance,' and 'between' are defined.

But what about propositions which refer to qualitative complexes in first-person experience? That is to say, what about propositions such as these: "The direction taken by the following two lines, __________ __________, is exactly the same"; "The experienced quale of the 'red' here differs from the experienced quale of the 'green' there"; "I have a headache."?

Consider the proposition concerning the two straight lines first. If it is taken to be a statement concerning two physically real "lines," i.e., if it is a statement concerning the ink marks on the paper, then this proposition is obviously not a self-evident truth; for it may be false. Two lines which *appear* to have the same direction may *actually* differ in direction—the difference being so small that it escapes casual observation. If perceptual impressions could always be relied upon to disclose the actual state of affairs, i.e., if judgments of perception were self-evident truths, much of the painstaking work now devoted to the devising and development of methods of measuring and checking perceptual impressions would be superfluous. But scientists do not find it so (cf. Blanshard).

[2] The specific problem of the law of excluded middle as encountered in mathematics and symbolic logic will be discussed in a later chapter. It affects in no way the present argument.

Let us suppose now that the proposition under consideration refers only to the experiential quale of the visual impressions. It is then of the same type as is the proposition concerning the difference in the experienced qualia of "red" and "green." Does this restriction to the realm of sense-data entail the self-evident truth of our propositions?

On the face of it it seems obvious that, since "red" and "green" look different to me, the proposition asserting this difference is true, because to me, as the one who experiences the difference and who asserts the proposition, the proposition is self-evident. But is this really all there is to it? If it is, then what does 'self-evident' mean in this connection? If it means anything at all, then, it seems to me, it must mean that somehow the sense-data themselves, as "given" or "brute" facts of experience, indubitably verify the propositions pertaining to them. And if this is what is meant by 'self-evident,' then numerous problems remain unsolved. What, for instance, are the "given" or "brute" facts of sense-experience?

In order to simplify matters, let us consider only those experiential situations in which error seems impossible. Propositions such as "I now see a red," "I now taste a bitter," "I now hear C#," indicate what is meant; and so do propositions such as "I have a headache," "I am afraid," "I feel nauseated." Does not the fact that I actually *see* red, *taste* bitter, or *have* a headache verify, immediately and directly, the proposition asserting this fact? And is not the proposition true because it is self-evident? Is it not true simply because *its very meaning is the fact as experienced?*

On the face of it we seem to have found here an indubitable certainty of elementary propositions which might well be called "self-evidence"; but only on the face of it.

When I assert that "I see a red" or that "I have a headache"—no matter how simple my proposition may seem—it yet refers to something which goes beyond a mere datum, beyond a "given" or "brute" fact. Who is the "I" who *sees* red or who *has* a headache? Do not the words 'see' and 'have' designate relations which are not simply part of the qualia of "red" or a "headache"? Does not the indefinite article in the proposition indicate class-membership? Is not "red" a specific kind of color quale specifically related to "orange" and "green" and "sound" and "taste"? Is not a "headache" a special kind of pain felt in a special part of my body? Are all of these relations "given" in the sense-datum itself? If not, then what actually is the "brute fact" which directly verifies the propositions in question?

Is it at all possible to pick out one item from the list and to say that *this* is it, or *this*? Are not rather all of the items mentioned necessary for the verification of my propositions? And are they not necessary in integrated unity?

But let us simplify the situation still further by excluding from it everything but the "red" or the "ache" or whatever specific "quale" we want to consider. Our proposition describing this remainder may then be stated in some such form as "This is red" ("There is an ache") or "Here now red" ("Here now aching") (cf. Blanshard). Have we now attained complete simplicity? Have we found the "brute fact" which directly verifies our proposition and makes it self-evident? I doubt it. That the simplicity of such particular experiences as "here-now-red" is apparent rather than real may be gathered from the fact that a complicated scientific theory of space-time is involved in any fully warranted assertion about the experience. Nor is this all.

All propositions dealing with sense-data are fundamentally synthetic. In and through them a particular "something," referred to as "this" or identified by "here now," is subsumed under a universal, be it "red" or "ache" or anything else. The very act of identifying a datum as "this" enmeshes it in the context of meaning and thought; and it is this context, and this context alone, which gives significance to the proposition in question.

In order to know that "this" is red or that "this" is a headache, I must already know the meaning of 'red' and of 'headache'; i.e., I must be in a position to relate the "this" to a whole class of similar experiences, and must contrast it with others. And in this process I may go astray. Where, for example, does "red" end and "orange" begin? Where is the line of demarcation between an "ache" and no ache at all? And is it not possible that I mistake for a "toothache" the pain which results from an infected sinus?

It is, of course, true that, as a rule, I go astray in my judgments concerning sense-data only at certain critical points. Is a given color "green" or is it "blue"? Ordinarily I may have no difficulty classifying it properly. But if the color in question is a "blue-green," or if light conditions obscure the hues, does not my judgment falter? Where is the self-evidence of the proposition upon which my decision can rest unfailingly? Or, to use another example, is the sound I now hear really *C#*? That is to say, can I always depend upon my "absolute" sense of pitch? To raise such a question is to answer it in

the negative; for the truth of the proposition "I now hear C#" is never self-evident. It is established only through the inspection of "confirmatory" evidence (cf. Blanshard; Ewing).

Now, if some propositions pertaining to sense-data are demonstrably not self-evident, is there any reason to believe that some of them are? Is there, in other words, a demonstrable difference between the two types—a difference which is more than a matter of degrees? To this question, I believe, the answer must also be negative; for no such difference is directly apparent in first-person experience and none has ever been established through irrefutable argument. At most it may be said that we accept certain propositions pertaining to sense-data as true because, *to us,* they identify or describe a state or condition which we actually experience at the time; because, in our first-person experience, they *correspond to the facts.* Such a statement, however, leads us at once to the "correspondence theory" of truth.

The Correspondence Theory

In its broad and elementary form, the "correspondence theory" of truth asserts that "a proposition is true if, and only if, it corresponds with reality." However, in this form the theory is untenable, for its defects are numerous and decisive.

To begin with, it is not at all clear what is meant by "correspondence with reality." If the phrase means a correspondence between the meaning of a proposition and the referent intended, then the issue involved is not one of truth or falsity, but concerns the correct or incorrect use of words. It is a problem in semantics, not one in epistemology; and as such it does not concern us here (cf. Blanshard).

If the phrase in question means that my belief in, or acceptance of, a proposition is in harmony with the actual truth-value of the proposition; that is to say, if it means that I accept or believe a proposition when it is true and reject or disbelieve it when it is false, the issue is again not one of truth or falsity but pertains to the soundness or reliability of my judgment. It is of psychological rather than epistemological import.

Finally, if the phrase "correspondence with reality" means that my "ideas," as mental events, correspond to "things," as non-mental entities, in a way similar to that in which a portrait of a person "corresponds" to that person, or in which a map "corresponds" to the geographical features of the region it represents, then it can be

shown that while the phrase does indeed define a criterion of truth, the criterion thus defined is impossible of application unless it is radically restricted in scope.

In order to ascertain whether or not a portrait "corresponds" with the person it portrays, i.e., in order to ascertain whether or not the "picture" is a *true likeness* of the "original," I must compare the two. Reasoning from facile but false analogy, advocates of the correspondence theory maintain that, in a similar manner, I can ascertain whether or not my ideas "correspond" with reality by comparing them with the actual things. If they are "true likenesses" of the things, my ideas are true; and if they are not such "likenesses," they are false.

This argument from analogy is fallacious because there is no way in which I can compare the things, as non-mental entities and *as they are apart from my ideas*, with my ideas about them. Things, as they are in themselves, are not accessible to me in the same way in which ideas are; and if they were, I should know them as directly and immediately as I know my ideas and should therefore no longer depend for true knowledge about them upon a comparison of my "ideas" with the "things."

The correspondence theory, in its broad and unrestricted sense, breaks down simply because it demands that I compare something given *in* my experience (the idea) with something which *is and remains outside* that experience (the "real" thing); and such comparison is impossible. All "comparing" takes place only within first-person experience and, as a consequence, only "ideas" can be compared with one another. I cannot compare my idea of the Grand Teton, for example, with the mountain itself as it exists outside my experience; I can compare it at best only with the perceptual experiences which I have when I believe that I see the mountain.

As applied to propositions dealing with things of the external world the correspondence criterion fails, furthermore, because it presupposes that we have knowledge of the things which is independent of the integrative interpolation of first-person experience, when, as a matter of fact, no such knowledge is available. In other words, the criterion fails because it implies that we adapt our ideas to "things as they are" when actually our knowledge about things—the very idea even that there are physical entities which transcend my first-person experience and yours—rests upon and presupposes

the validity of thought and the truth of at least some propositions dealing with first-person experience.

But what about the modified or restricted "correspondence theory" according to which propositions pertaining to sense-data are true if what they assert "corresponds" with the facts of sense-data *as experienced*? What about the contention, in other words, that the simple ostensive propositions—such as "This is red," "This is bitter," "This is C#"—may be verified by their "correspondence" with our actual experience of the respective sense-data? This restricted "correspondence theory" avoids at least the most crucial mistake of the broader view. The reference to a reality which remains outside all possible experience has been eliminated and all of the elements which are to be "compared" with one another are assumed to be immanent in first-person experience; we can be aware of them at the same time and in the same sense (cf. Sellars).

The advantage thus gained for the "correspondence theory" is, however, more apparent than real; for the theory, in this restricted sense, is insufficient as a criterion for all those propositions which do not ostensively deal with sense-data. It is insufficient, in other words, for our largest and most important body of knowledge.

Scientific knowledge in particular pertains neither directly nor even primarily to immediately experienced sense-data. The verification of the propositions or laws of the sciences is achieved, not so much through a "comparison" with sense-data, but rather through propositions and chains of propositions which only at some ultimate point may pertain to sense-data. In modern physics, for example, verification consists essentially in disclosing the *inner consistency* of a formal system of laws, and in making evident the general agreement of this system with an *integrative interpolation* of certain data (and not only sense-data) of first-person experience.

What is true in physics is true in the other empirical sciences as well. The criterion resorted to is "coherence of ideas" rather than "correspondence" between ideas and some "given" sense-data. However, we shall not press this point at the present; it suffices for our purposes to have pointed out here the inadequacy of the criterion of "correspondence."

We ask next, Is it a fact that all simple ostensive propositions are true because they "correspond" directly with the data of immediate sense experience; and is it a fact that at the level of sense experience "correspondence" is the sole and sufficient criterion of truth?

In Chapter III enough evidence has been produced, I believe, to show that the "simple and isolated sense-datum" is never "given" in experience; that, on the contrary, this "datum" is obtained only through a process of abstraction which isolates it from the context of first-person experience as a whole. This fact obviously leads to some doubts concerning the significance or reliability of the "correspondence" criterion; for if that criterion implies that our simple ostensive propositions are true only because they "correspond" with some "data" *which we ourselves have delimited through a process of abstraction*, then the "standard of cōmparison," so essential to the correspondence theory, is by no means fixed or "given" but is conditioned by, and dependent upon, the very same processes of thought for which it is to be the standard. The argument, if pressed, turns into a vicious circle.

Actually, of course, the presence of sensory elements in our first-person experience cannot be denied. The terms 'red,' 'bitter,' 'hot,' 'C#,' 'pain' designate *experiential qualia* which cannot be reduced further and which cannot be derived from other factors in experience. But the moment we identify a certain quale as "red" or as "pain" —and only when it has been so identified can it serve as a "standard of comparison"—we view it in relation to other qualia of "its kind" and in contrast to qualia *not* of "its kind"; that is to say, the moment we identify it, we no longer deal with it as a mere sense-datum. The concept employed in its delimitation identifies it as a *member of a class.*

Now, to identify a specific quale as a member of a certain class involves, in principle, two things. It involves, in the first place, a reference to the experiences of the past which provide the basis for my understanding of the term employed. In this sense, my assertion, "This is red," means, "This is like such and such other experiences which I have had." Whatever "comparing" is involved in this recognition pertains, in the last analysis, to a "present datum" and my memory of "similar data previously experienced." If in the past I have called such and such qualia "red," then I must call the present quale by the same name. It is the context with those other experiences which determines whether or not my present assertion is true. And this, it seems, is a matter quite different from what the correspondence theory implies.

In the second place, my identification of a specific quale as a member of a certain class pertains also to the future. That is to say,

when I say with reference to a particular quale, "This is red," I expect that whenever I actually do experience the intended quale it will be such as I have identified as "red" in the past. The meaning of concepts thus transforms my memory of previous experiences into an anticipation of the future; and the proposition expressing this anticipation is true if, and only if, the future experience actually is of the kind I expect; i.e., if it is similar to the experiences of the past which have given meaning to my concept. Again, it will be noted, the criterion of truth is context with other experiences rather than a "correspondence" between my proposition and a "given" datum.

It may be argued, however, that the kinds of propositions so far considered are not the most elementary available and that only the latter imply the "correspondence" required as a criterion of truth. We presumably encounter these "elementary" propositions when we identify some experiential quale not previously observed as a specific "this." The quale in question may be a color never previously seen by anyone, or a sound previously unheard, or a sensory quality *sui generis* which is neither color nor sound nor taste nor smell nor anything else so far known to man; but if we say of it, "This is andimo"—using a new name to identify the new quale—are we not then employing a proposition which is true only because it "corresponds" with the quale itself? The answer, of course, is that in this case we employ no proposition at all; for either the statement, "This is andimo," is meaningless because it involves an undefined term, or it is equivalent to the statement, "I shall henceforth identify this new quale by calling it andimo," and it is then a resolution to use a certain word in a certain way; i.e., it is a definition rather than a proposition. As definition, it is neither true nor false but indifferent to all truth-values.

There are, thus, no propositions of a type more elementary than those which we have already considered; and the truth of those propositions depends on the context of experience rather than upon any "correspondence" between an individual proposition and an isolated sense-datum. As a consequence of this dependence upon context, the propositions in question are not infallibly true; for I may be mistaken in relating a specific quale to a class A rather than to a class B. I may, for example, identify it as "red" when I should have identified it as "orange." The error is not just a "breach of custom" in the use of words; it is a mistake *pertaining to fact* and is therefore one rendering false the proposition, "This is red." But the "fact" it refers to is not an isolated sense-datum. It is, rather, the

context of experience in which sense-data occur as differentiable classes of qualia.

One last point may be noted. In the actual process of scientific inquiry, propositions pertaining to "facts" are important only because of the bearing they have upon theories and newly formed hypotheses. If the "facts" support the hypothesis, they are accepted as "confirming the theory"; but if they conflict with it, they do not always lead to the rejection of the hypothesis. On the contrary, the facts themselves may be "broken down" in the light of the hypothesis and may be "re-interpreted" until agreement with accepted theory has been reached. That is to say, observation and experience are never taken as providing irrefutable proof of the truth or falsity of propositions —as they ought to be, if "correspondence" with perceived facts were the test of truth. What actually happens is that we accept the evidence of "facts" only if the consequence of rejecting the propositions which they support is more disastrous to our body of knowledge than is the consequence of accepting them. Context and coherence, in other words, is the last court of appeals even here (cf. Waters).

The Pragmatic Theory

In at least one of its forms the "correspondence theory" of truth assumes that we have definite *images* of things, and that "truth" is the correspondence of these images with the things of which they are the images ("copy theory" of truth). But, as Spencer and Peirce have noted before, many of the ideas presented in our propositions have no "pictorial" meaning. We have, for example, no "images" of mass, gravitation, electricity, and magnetism; of free will, justice, social order, or of God; nor can we conceive genuine "pictorial images" of such matters. Our analysis of concepts has shown, furthermore, that *no* universals can ever be "pictured" *as universals*. Wherever images are associated with our concepts, they are unessential, if not irrelevant, to the meanings intended—unless, of course, the images themselves are the referents under consideration. The whole idea of "pictured" meanings is inadequate, if not misleading, and ought to be discarded. But what is to be put in its place?

Herbert Spencer maintained that an "unpicturable idea" has definite meaning if it leads to verifiable predictions. In extension of this principle, Charles S. Peirce asserted that the meaning of every idea may be found in the difference it makes in our experience or in the sense-effects to which it leads. If ideas make no difference in experience, i.e., if they have no discernible effects, they are meaningless;

and if they have identical effects, then they are the same ideas—regardless of what words may be employed in speaking about them.

Now, in so far as ideas make a discernible difference in my experience, Peirce argues, they also induce me to make specific adjustments. I act in response to them, and I conform in whatever I do to their implied suggestions. Ideas, in other words, are "plans of action." They have meaning only *as* "plans of action"; and belief in them "establishes in our nature a rule of action or . . . a *habit*."

If, for the sake of argument, we accept this "pragmatic" doctrine of *meaning*, then we are led, more or less logically, to William James's contention that a belief is *true* only if it guides us to success or if it establishes valuable habits, and that it is false if it leads us astray or develops destructive habits. We are led to the conclusion, in other words, that *"a belief is true if it works"* (cf. Blanshard).

This "pragmatic theory" of truth suffers, however, from an initial ambiguity; for it is by no means clear what is meant by "it works."

For instance, "it works" may mean simply that the belief in question leads to the perceptual experiences or to the pain or pleasure anticipated or predicted in the belief. In this sense, the verifiability of a scientific hypothesis may be said to be its *truth*.

But, "it works" may also mean "it is in harmony with other propositions." No belief can be taken entirely by itself. Isolated propositions do not constitute a body of knowledge. But neither do groups of contradictory or inconsistent beliefs constitute such a body. Hence, if a new belief is out of harmony with beliefs already accepted, either the new belief must be discarded or the old ones must be overhauled. Whichever alternative we choose, only that belief can be regarded as "true" which "works" in the sense that it establishes complete (or, at least, an increased) harmony of all accepted beliefs —including the beliefs of other people.

Finally, "it works" may mean that the belief gives desirable direction to our feelings and emotions. In this sense, other things being equal, a belief which gives comfort and stability and moral strength, a belief, in other words, which "enhances life" and, in general, makes for optimism, is to be preferred to one which tends in the opposite direction. It is in this sense that religious, moral, and metaphysical beliefs may be regarded as "true"—especially if experience shows that in the long run these beliefs promote the welfare of mankind as a whole.

Of these interpretations of the phrase, "it works," the first two are clearly the most important. The third is of no cognitive significance whatsoever; for it ultimately implies the destruction of all objective standards and the enthronement of a thoroughgoing relativism of subjective beliefs. Whatever contributes to the "abundance of *my* living," i.e., whatever "works" *for me*, is "true"! It is obvious that knowledge, as we understand it and as it permeates the whole of modern civilization, finds no support in such a criterion, and that the edifice of the sciences cannot be based upon the quicksands of such a doctrine.

The pragmatists themselves have resorted to this third interpretation of their criterion only when a given issue could not be decided on other grounds.

The first two interpretations of the phrase, "it works," deserve careful analysis. If they are combined so as to supplement each other, they are especially significant. For the present, however, the first of the two criteria, the appeal to "verification," is of primary interest; for the second, the criterion of "harmony with other propositions," is essentially an appeal to *context* and will therefore be discussed when we deal with the "coherence theory" of truth.

The pragmatist's appeal to "verification" is inseparably bound up with his thesis that "ideas are plans of action"; for, according to pragmatic doctrine, a proposition is "true" when our action, carried out in conformity with the idea or "plan" embodied in the proposition, actually leads to the result anticipated by the idea. It follows that the criterion of "verification" will be impaired if it can be shown that the meaning of an idea is not in itself a "plan of action" (cf. Hinshaw).

What, precisely, is a "plan of action"? What does the idea of such a "plan" involve? Let us examine a specific case.

Suppose that it is my "plan" to buy a new suit. That is to say, I desire to bring about a certain state of affairs not now existent (my ownership of a new suit) and I believe that through a certain action on my part (purchase) I can realize my goal. However, neither my desire nor my belief nor the combination of the two is in itself a "plan of action." Something else must come into the picture. I must *resolve to act* in the manner which I believe will lead to the desired result; i.e., I must resolve to purchase the suit. Without this resolve I have no "plan of action"; I have at best only a *basis* for such a plan (cf. Blanshard).

If this distinction between "basis for a plan" and "plan of action"

reflects—as I believe that it does—a legitimate differentiation in first-person experience, then it follows that the thesis which identifies the "meaning" of an idea with a "plan of action" is at least misleading. If I know, for example, the "nature of a typewriter," i.e., if I know what properties a "typewriter" has, and if I also desire a certain end (such as writing a letter), then my knowledge of the properties of a typewriter is a *basis* for a plan of action (although it is not the sole factor to be considered), but my "plan of action" itself is the *resolution to use* the typewriter (rather than a pen) for the purpose of writing the letter. Stated in general, our knowledge concerning the "nature of things," i.e., any proposition or "idea" which designates a set of properties or which defines a "thing," is a potential basis for a plan of action but is not that plan itself. To take it as such a plan is to take it for something which it can never be; for a proposition is not in itself a resolution; it is not a decision to act in a certain manner.

As basis for a plan of action an idea is, of course, relevantly connected with any plan based upon it. The plan is what it is, at least in part, because of the "idea" which serves as its cognitive basis. Hence, if our "idea" is true, the plan based upon it should have a good chance of succeeding. That its success is not fully assured is due to the fact that in this world of causal determinations and conflicting interests the execution of any plan depends upon other factors besides the cognitive basis. If the "idea" is false, the plan based upon it may yet succeed because of a chance constellation of causal factors which has little or nothing to do with the "plan" as such. The success or failure of a "plan," therefore, is no guarantee of the truth or falsity of the "ideas" which provide its cognitive basis. "Success" is not identical with "truth," nor is "failure" the same as "falsity."

And yet, if we wish to ascertain whether or not a given proposition is true, we must, as a rule, do something or perform some "operation"; i.e., we must manipulate other propositions (inference) or we must observe and handle things (experimentation). If the "operation" leads to the anticipated result, we regard the proposition in question as true; if it leads to results which are incompatible with what was anticipated or "predicted," the proposition is false. Scientific verification is always of this "operational" type.

Even so, however, the "pragmatic theory" of truth is hardly adequate; for it leaves unanswered one crucial question. If we can discover the truth or falsity of a given proposition only by acting upon

it or by performing some "operation," then how do we know whether or not the actual result of the "operation" is identical with the predicted result? To put it otherwise, How do we know that the proposition, "This *actual* result is identical with this *predicted* result," is true? Do we know this in turn only through some other "operation"? If so, then the same question returns with respect to the result of this new "operation"—and with respect to the result of *every* additional "operation" which we may perform. In other words, if the truth of a proposition can be ascertained *only* by performing some "operation," then an infinite series of "operations" must be performed before a single truth is established; and this is manifestly impossible. Exclusive reliance upon "operation," therefore, results in no truth at all. If, on the other hand, this regress *ad infinitum* is to be avoided, we must accept a criterion of truth which, at cardinal points in our search after knowledge, makes further recourse to "operations" unnecessary. The "pragmatic theory" does not provide this additional criterion (cf. Blanshard).

The Verifiability Theory

Logical positivists and empiricists have developed a theory which makes "verifiability" the criterion of truth.

In its early (and crude) formulation this criterion was identified with *actual verification*. A proposition was said to be true only when it *had been* verified, i.e., when an "operation" had been performed "demonstrating" its truth. If for theoretical or practical reasons actual demonstration was impossible, the proposition was regarded as "unverifiable" and was said to be neither true nor false.

This theory, strictly speaking, identifies "truth of" with "having demonstrated the truth of"—an identification which, if clearly understood, is completely arbitrary and is not justified by general usage of the terms; or which, if not clearly understood, is an intolerable equivocation and a source of error and confusion. Logical positivists, therefore, soon modified their theory. Instead of using "actual verification" as the criterion of truth, they now speak of "verifiability" and, more precisely, of "verifiability *in principle*." A proposition is said to be "true" when it is "verifiable in principle"; i.e., when we know the conditions which, when realized, will make "verification" possible (cf. Ayer).

It is necessary, however, to impose at once certain restrictions upon this general statement; for the "conditions" which we regard in

any given case as making "verification" possible must contradict neither the laws of logic nor any known law of nature. They must, in principle, be realizable in the world as we know it. The proposition, "There are mountains on the other side of the moon," is "verifiable" in this sense; for we know exactly what would be necessary to verify it, and these known conditions are, in principle, not unrealizable in the world in which earth and moon exist; i.e., they do not contradict the known laws of that world. On the other hand, the proposition, "I remember best the things which happened the week after next," is "unverifiable"; for, given the conventional meaning of its terms, the proposition asserts a logical impossibility, and the condition of "living backwards" which it assumes is irreconcilable with the laws of nature known to us. The fictitious conditions of a "world-through-the-looking-glass" can have no bearing upon the truth or falsity of a proposition. If they had, the criterion of "verifiability" would lose all significance, for it would then entail the question-begging assertion that we should be able to verify any given proposition if we were in a position enabling us to verify it. No limits would be imposed upon our imagination or upon our ability to *invent* "conditions," and no proposition whatever would remain "unverifiable" (cf. Ducasse, 1941).

But if the criterion of "verifiability" is restricted as indicated, it is still far from being adequate or sufficient; and this for two reasons. (1) It presupposes a body of established laws and of "true" knowledge about the world which, in turn, must find justification in some form of "verifiability." And if the same restrictions are again imposed (as they must be), then there must exist a still different body of knowledge established by some still more antecedent form of "verifiability," and so on without end. It is difficult to see how under conditions involving such a regress there could ever arise a body of "true" knowledge.

(2) The criterion of "verifiability" leads to an infinite regress also in a different way—and in a way which is strikingly similar to that involved in the "pragmatic" theory of truth. If a proposition, P, may be regarded as true only when we know the conditions under which it can be verified, does it not follow that any proposition P′ which asserts the truth of P, i.e., any proposition of the form, "P fulfills all conditions of verifiability," can be regarded as true only if the conditions of *its* verification are also known, and so on *ad infinitum*?

If either regress into infinity is to be avoided, at least some of our

propositions must be of a type such that their truth no longer depends upon "verifiability" or "conditions of verifiability" but becomes evident in some other and more direct way. The criterion of "verifiability" must be supplemented by considerations of a different kind (cf. Blanshard).

Logical positivists and empiricists may be willing to admit (1) that our criticism is relevant as far as propositions of an *essentially hypothetical* nature are concerned, and (2) that the criticisms so far developed would be decisive if *all* propositions were of this type. They will maintain, however, that some propositions are not hypothetical and that "verifiability" of these propositions means something quite different from what we have discussed up to now. Let me try to make this point clear.

It must be noted that some propositions can be "verified" only in an indirect way; for their complex terms designate nothing that is "given," directly and immediately, in first-person experience. Propositions of this type entail, however, more elementary propositions; i.e., they entail propositions which are "irreducible" or "basic," and which can be "verified" by immediate experience (cf. Neurath). For instance, the proposition, "This is a tree," entails certain statements concerning sense-data: "If this is a tree, then I shall see certain colors and shapes and, upon touching it, I shall have specific tactual experiences." The sense-data thus referred to are objects of immediate experience. If I encounter them in my own experience they "verify" for me, or confirm, the original proposition. But the whole process indicates that the proposition, "This is a tree," is essentially a hypothesis. It can be accepted as true only if we know the conditions under which we can actually experience the sense-data referred to in the "basic" statements which it entails. If all propositions were of this type, the criticisms so far advanced against the "verifiability" theory of truth would be final and unanswerable.

The "basic" statements, however, are intended to be of a different type. Their "verification" is assumed to be direct and not mediated by other propositions. Some philosophers, in order to eliminate all possible doubt as to their meaning, speak of "confirmation" (and "disconfirmation") when the propositions under consideration are essentially hypothetical, and of "verification" (and "falsification") when the propositions in question are "basic" statements. A proposition is said to be "confirmed" when the basic statements which it entails have been "verified"; it is "disconfirmed" when the same basic

statements have been "falsified." And, correspondingly, a proposition is said to be "confirmable" if we know (1) the basic statements which it entails and (2) the conditions under which these basic statements may be "verified." It is "disconfirmable" if we know (1) the basic statements which it entails and (2) the conditions under which these basic statements may be "falsified."

If this is at all an adequate statement of the meaning of "confirmation" ("disconfirmation") or of "confirmability" ("disconfirmability"), then it is evident, I believe, that both "confirmation" and "confirmability" are essentially matters of context with other propositions —of context, namely, with "basic" statements. And to the extent to which this is so, context or "coherence" becomes the criterion of truth. "Verifiability" is only an adjunct to the contextual pattern.

The matter seems to be quite different in the case of all "basic" statements. Propositions of the type, "This is green," "This is sour," "This is C#," seem to be "verifiable," directly and immediately, through our experiencing, here and now, the specific qualia referred to. But if this is what is meant by "verifiability"—the possibility of noting in our first-person experience the actual presence of the intended quale—then the criterion of "verifiability" is essentially identical with the ultimate criterion of "correspondence"; and the arguments advanced against the latter hold good also against the former.

There is no escape from context or "coherence." The isolated "basic" statement is as much a fiction as is the isolated sense-datum or the isolated "thing."

The Coherence Theory

If all our analyses of truth lead to context or "coherence," and if there is no escape from "coherence," does this perchance mean that "coherence" itself is the criterion of truth? To this question the "coherence" theory gives an unqualifiedly affirmative answer; and its advocates advance negative as well as positive arguments to support their contention (cf. Blanshard). The negative arguments rest upon the generally admitted fact that inconsistencies within and among propositions are indicative of error or non-truth. If it be asserted, for example, that the earth is round, and also that it is flat, then this contradiction or "incoherence" indicates that there is error or non-truth somewhere. Of two contradictory or inconsistent propositions at least one must be false; or, what amounts to the same thing,

true propositions must be consistent with one another. This means that they must "cohere."

On the positive side it can be pointed out that the *ideal of the system*, the ideal of an integrated "coherent" body of laws and principles, is the ultimate goal toward which we are striving in cognition, and that philosophers and scientists alike, working on the elimination of inconsistencies, try to achieve a systemic "coherence" of all knowledge.

These arguments, admittedly, do not tell the whole story. Nor do they indicate specifically what "coherence" means. It may be granted that inconsistency means error or non-truth; but we cannot infer from this that consistency means truth. The "coherence" criterion, therefore, must mean something which includes consistency but which also goes beyond it. I take it to be a type of interrelatedness of propositions which can best be described as *systemic entailment.* In its rigid form this interrelatedness would be comparable to the interdependent system of theorems which constitutes Euclidean geometry. In its scope, however, it would encompass all possible "true" propositions and would leave no aspect of experience unintegrated. No "true" proposition would remain outside the "system."

Now, if "coherence" is taken in this comprehensive sense of "systemic entailment," then certain difficulties are at once apparent. The relation of "entailment," for example, is not in itself a guarantee of truth; for if the truth of a given proposition, *p*, entails the truth of some other proposition, *q*, then it is also a fact that the falsity of *q* entails the falsity of *p*. "Entailment," in other words, is reconcilable with falsity as well as with truth (cf. Ducasse, 1944).

Furthermore, if a "system" of propositions, A, is so constructed that *p* entails *q*, *q* entails *r*, *r* entails *s*, and *s* entails *t*, then, because of the very nature of the entailment relation, there exists also a "system," B, within which non-*t* entails non-*s*, non-*s* entails non-*r*, non-*r* entails non-*q*, and non-*q* entails non-*p*. If A is a "coherent" system, then B also is one, and for the same reason.

However, all propositions included in B are the contradictories of their respective counterparts in A. Since of two contradictory propositions at least one must be false, it follows that at least one of the two systems must contain false propositions. If this is so, and if both systems are equally "coherent," then "coherence" taken by itself is inadequate as a general criterion of truth.

In their endeavor to escape this criticism, the advocates of the

"coherence" theory of truth may now maintain that the preceding argument rests upon the fictitious assumption that both systems, A and B, are of equal experiential significance. They may maintain, in other words, that if system A actually integrates our first-person experience—either the whole or a part of it—then system B will fail to do so; and that this fact is decisive. The real issue, then, is not the mere *formal* equality of contradictory systems, but their effectiveness in the integration of experience. "Coherence," in this sense, means "integration of experience." The truth (or falsity) of a proposition is determined by the extent to which it contributes to a "coherent" view of experience, or, in the negative sense, by the extent to which its denial would disrupt such a view.

An obvious answer to this new argument is the contention that unless the truth-value of some propositions is known without recourse to "coherence" we have no assurance that any particular "system" of propositions actually integrates experience. In other words, we encounter once more the need for "basic" statements which link "systems" of propositions with the "facts" of experience, and which thus warrant the assertion that our "system" integrates experience.

But "basic" statements or "elementary" propositions, so we have already seen in previous sections, require context for their verification. Our whole argument, therefore, seems to be circular and the criterion of truth as illusive as ever. In view of this situation it becomes necessary to restate the whole case, and to do so irrespective of all traditional theories.

Restating the Case

So far our discussions have suffered from an ambiguity inherent in the very problem of truth; for we have raised the questions of truth and the criterion of truth in a manner which implies that they are the same for all types of propositions, and this assumption is unwarranted.[8]

Consider, for example, the following propositions:

1. A is not non-A.
2. X is either A or non-A.
3. All roses are plants.
4. No squares are round.
5. The sum of the interior angles in a triangle is equal to two right angles.

[8] Cf. B. Blanshard, *The Nature of Thought*, II, Chapters XXV to XXVIII.

6. $c^2=a^2+b^2$.
7. This is a tree.
8. Lincoln is the capital city of Nebraska.
9. This is red.
10. This is sour.

In some way all of these statements may be regarded as true; that is to say, we may accept them as "not erroneous." But even a cursory examination reveals that they are not all "conformable to fact," or at least that they are not all "conformable to fact" in the same sense. The "warranty" of these beliefs is specifically different with respect to different propositions.

Propositions (1) and (2), for instance, find their warranty in the fact that their denial paralyzes all thought. If these propositions are not "true," then intelligible thought is impossible and all thinking must come to an end. Propositions (3) and (4), however, cannot be proved in this manner. Their warranty lies in the definitional interrelation of their terms. Proposition (3) is "true" because (a) the meaning of 'plant' is included in the meaning of 'rose,' and (b) the proposition itself asserts this inclusion. Proposition (4) is "true" because (a) the meaning of 'round' is, by definition, excluded from the meaning of 'square,' and (b) the proposition itself asserts this exclusion.

Proposition (5) is "true" because it is an integral part of Euclidean geometry, i.e., because it is a theorem which can be deduced with logical rigor from the postulates and definitions that provide the foundation for Euclidean geometry. Proposition (6), in its geometrical sense, can be similarly derived from the same set of postulates and definitions, and in its algebraic sense it follows from a comparable set of "assumptions." The warranty of propositions (5) and (6) is therefore furnished by the systemic whole of which they are integral parts.

Propositions (7) and (8) are essentially "hypotheses" the "truth" of which can be established only by "confirmation." Propositions (9) and (10), finally, are "basic" statements pertaining to sense-data. Their warranty may be found in the immediately experienced qualia of first-person experience.

If, in view of this variety of warranty, the term "truth" is to be retained, then this term cannot really mean "conformable to fact"; for only propositions (7) to (10) can in some sense be said to be "conformable to fact." The other propositions are "true" without

reference to "facts." Their warranty is given in types of relations which are independent of "things" and "sense-data" and other concrete contents of first-person experience.

Henceforth, we shall identify "truth" with "warranted belief" and, therefore, with "knowledge." "Truth" is *warranted belief*, and "warranted belief" is *truth*. The *modes* of warranty are then secondary, and the degree of warranty, i.e., the certainty of knowledge—varying as it does from unquestionable certainty of a belief to an almost complete lack of support for a belief—becomes an intelligible and manageable factor.

As far as the *modes* of warranty are concerned, propositions (1) to (4) represent *semantic and syntactical truth*. They represent, in other words, a type of knowledge the warranty of which is given in the meaning of terms and in their syntactical combinations. Propositions (5) and (6) are examples of *systemic truth*. That is to say, they exemplify knowledge which derives its warranty from the logical interdependence of all integral elements in a deductive system. Propositions (7) to (10) are instances of *empirical truth*. Their warranty may be said to stem from a "confirmatory" relation to specific qualia of first-person experience.

Closer inspection reveals that the three types of truth or modes of warranty here referred to share at least one characteristic feature. In one form or another they all involve an *appeal to coherence*. In the case of "systemic truth" this dependence upon coherence is most obvious; for warranty here means "being logically demonstrable" or "being derivable from" stipulated premises. The "truths" of pure mathematics, for example, are all theorems derivable from certain definitions and postulates. Their warranty lies exclusively in this relation of logical dependence and entailment, i.e., it lies in the systemic context, in the logical coherence of the system.

In the case of semantic and syntactical "truths," the appeal to coherence is also evident. Propositions of the type exemplified by (1) and (2), i.e., propositions which constitute an indispensable presupposition of intelligible thought, must be accepted as true because the consequence of their assumed falsity paralyzes all thought and makes thinking impossible; whereas propositions of the type exemplified by (3) and (4), i.e., so-called analytic propositions, must be accepted as true because of the interdependence of meanings and because of the rules of logic and grammar (the rules of syntax) which govern their construction. They are true, in other words, be-

cause of the language employed. Arguments based upon consequences or upon the nature of language are, however, appeals to context or coherence; for they appeal to a system.

When we turn to empirical truths, it may be helpful to keep in mind the distinction between propositions—such as (7) and (8)—which are of an essentially hypothetical nature, and propositions—such as (9) and (10)—which are "basic" statements. That the truth of the former can be established only through an appeal to context or coherence is obvious; for "confirmation" through the verification of consequents is unthinkable without recourse to coherence. A proposition of this type can be regarded as "true" only when it entails "basic" statements which are "true," i.e., when it "coheres" with these statements. If I assert, for example, "This is a blue jay," the proposition entails numerous other propositions, some of which pertain to the nature of birds and of animals in general, while others pertain to the nature of flight, to color and plumage and to the piercing shriek of the jays. If all these entailed propositions—or still other propositions entailed by them—can be accepted as "true," then the original proposition, "This is a blue jay," is "confirmed" and may be accepted as true, i.e., as warranted belief. The context or coherence with entailed true propositions thus establishes the truth of the original assertion. The more complex that assertion is, that is to say, the longer the sequence of logical entailments which connects it with "basic" statements, or the further removed from "direct observation" it is, the more evident is the fact that its truth depends on coherence (cf. Blanshard).

But what about the "basic" statements? What, in other words, about propositions—such as (9) and (10)—which are concerned directly with qualia of first-person experience? The answer to this question has for the most part been anticipated in our discussions of "self-evident" truths and of the "correspondence" theory of truth. The following facts, brought out in those earlier discussions, may be summarized here for the sake of clarity: (a) All propositions dealing with sense-data are synthetic. In and through them a specific "this" is subsumed under a universal. (b) The very act of identifying a datum as a "this" enmeshes it in the context of meaning and thought; for such an act involves discrimination and selection. (c) At least some propositions pertaining to sense-data are demonstrably not self-evident but can be proved true only through "confirmatory evidence," i.e., through consistency with other propositions. (d) All propositions

pertaining to sense-data are, in principle, subject to error and may be false. They require, therefore, in principle, confirmatory evidence. (e) This evidence is their consistency with other propositions pertaining to the qualia of immediate experience. All of which means that here, as in the case of all other types of propositions, the ultimate criterion of truth is coherence and contextual consistency (cf. Blanshard).

It may be objected that this interpretation of truth can never yield truth because we have no secure anchorage to which to tie our "merely coherent" system. Professor Ducasse, for instance, would make this very point. I believe, however, that the objection is not fatal to the coherence theory. The quest for an "ultimate" or absolutely indubitable point of support is misdirected in matters of factual knowledge. All knowledge of things is relative, and the truth it embodies is relative truth. But this is exactly what we should expect on the basis of the coherence theory.

The argument that "mere coherence" can never be a test for empirical truth would unquestionably be decisive were it not for the fact that propositions pertaining to immediately experienced sense qualia enter into the context of coherent propositions. The "validation" of each and every one of these "basic" propositions can be achieved, in principle, only through context with other propositions of their kind. That is to say, only when all propositions pertaining to specific sense-data form a self-consistent "set" can each one of them, taken as individual, be regarded as true. The objection to the coherence theory of truth, I believe, stems from the conviction that isolated propositions, *qua* isolated, must be "verifiable." This conviction, however, rests upon an untenable assumption; for "isolated" propositions, being fictitious abstractions, do not exist in intelligible thought. Each and every statement that conveys any meaning at all does so only because of a context of which it is an integral part. Its context, i.e., its coherence with other propositions, cannot even be denied without being reaffirmed in the denial; for the denial must be either an appeal to an entailed contradiction or an appeal to an entailed incoherence. But in so far as it is an appeal to an entailment, it is also an appeal to coherence or context.

Concluding Remarks

It is clear, I believe, that the coherence theory of truth is in complete harmony with the thesis, developed in Chapter II, that meaning

is essentially a matter of context, and also with the thesis, developed in Chapter III, that knowledge results from a systemic integration of first-person experience. If a progressive integration of the qualia of immediate experience and the construction of a "pattern" of interdependent "things" are the cardinal features of cognition, then it would be strange indeed if coherence and systemic context were not the criteria of truth.

More important is, of course, the fact that the coherence theory is in harmony also with the practices and procedures in science and philosophy. Philosophical "dialectics," as a method of clarifying ideas, is without question a matter of coherence; and scientific methods of verification are impossible without recourse to context. The "data of observation" are already conceptually integrated when they become evidence for or against a given theory, and their integration means that they have been taken up into a context of ideas. Prior to such integration no "datum" is or can be "evidence," for it does not even exist as a distinctly discerned and identified "this." An example or two will make this clear.

The measurable change in the mass of a body, though in itself a fixed and determinable quantity disclosed by observation, verifies either the proposition, "This body is at rest," or the proposition, "This body moves with such and such an acceleration." Einstein's *principle of equivalence,* i.e., the crucial principle of the general theory of relativity, clearly shows that what is important here is not the observed change in mass as such but its conceptual integration as "gravitational" or "inertial" mass, respectively.

If this example seems to involve too much "theory," consider the pattern of dark and light rings on a photographic plate which the physicist obtains when he sends certain rays through a thin celluloid film. This *visual* evidence verifies either the proposition, "The rays in question consist of waves," or the proposition, "The rays in question consist of particles." What we *see* is in either case an unchanging pattern of dark and light rings in concentric arrangement; the difference lies in the conceptual integration of this pattern, in its identification as a specific kind of a "this." If it is taken to be an "interference" pattern, then the rays involved are undulatory X-rays; but if it is taken to be a "diffraction pattern of electrons," the rays are streams of corpuscles. Evidently the sense-data as such decide nothing here. Only our conceptual integration of what we "see" is at all significant

and has any bearing upon the truth or falsity of the respective propositions.

If sense-data as such, i.e., separated from all conceptual context, were a sufficient criterion of truth, the whole procedure of the sciences might be different; for many of the troubles which now beset scientific inquiry arise from the fact that the "same" sense qualia are reconcilable with, or "verify," many different propositions. Only the coherence theory of truth, depending as it does upon the conceptual integration of sense-data, gives an adequate account of scientific procedures and provides a criterion of truth which remains dependable despite the inherent ambiguities of "mere" sense qualia.

The ideal, the goal, toward which all cognition moves, is the achievement of one comprehensive system of logically interdependent propositions—a system, however, which at all crucial points implies propositions of the form, "This is such and such a quale of first-person experience." To understand anything means to integrate it with this system or at least with that part or fragment of the system which so far has emerged as the result of our integration of experience. If a new experience cannot be integrated with the accepted fragment of the anticipated system, i.e., if it is inconsistent with it, if it does not "cohere" with it, the fragment must be re-integrated until consistency and coherence have been achieved. That such "revolutionary" experiences do occur is evident from the history of science; that they provide stimuli for cognitive processes is also evident from that same history. The discoveries of electricity, of the constancy of the velocity of light, and of radioactive disintegration of atoms illustrate the point.

Opponents of the coherence theory may now argue that the very fact that "revolutionary" discoveries may lead to a discarding of theretofore accepted systems of interrelated propositions proves that coherence is not really the criterion of truth; for if it were, the coherent system should prevail against the isolated proposition stating the new observation.

The force of this argument is more apparent than real. The "isolated" proposition does not exist. *The* "proposition" asserting the new discovery is in itself a contextual "complex," a fragment of a system; for it entails numerous propositions of different kinds, and is consistent with still others. The issue, therefore, is this: Which is more disastrous to our ideal of cognition, to adhere to the accepted fragment of an *anticipated system* and to exclude the set of proposi-

tions which contradicts the systemic projection, or to modify our projection of the system as a whole to such an extent as to accommodate within it both the hitherto accepted fragment of knowledge *and* the newly indicated fragment? So long as we are not in possession of the whole truth, i.e., so long as we have not achieved a complete and coherent integration of the whole of experience, so long as the "system as a whole" is but an anticipation or projection, there is nothing sacrosanct about any particular fragment of the projected system. That is to say, until we have achieved complete and total integration of experience, any partial integration can be accepted only subject to future revision. The idea of coherence, far from failing as a criterion in this situation, becomes itself one of the mainsprings compelling us to revise and revise again the fragmentary knowledge of the moment until the whole is within our grasp (cf. Blanshard).

Finally, if integration of first-person experience is the only available way to an understanding of reality, as I believe that it is, then only the completed system of interdependent propositions which integrates the whole of that experience and which excludes nothing which is in any way present in that experience, is at all adequate as a description or interpretation of reality. It, and it alone, is the true story of the real. And in this sense truth and reality stand revealed as projected systems; the completion of one is the fulfillment of the other. Every fragmentary achievement in the system which is truth reveals at least that much of the real; and every understanding of the real is disclosed in a set of propositions which foreshadows the completed system of truth.

PART III

FORMAL KNOWLEDGE

CHAPTER V

LOGIC

In so far as knowledge pertains to subject matter it has its *material* aspects; but in so far as it is formulated in propositions and chains of propositions, it has its *formal* aspects as well. These formal aspects of knowledge, the logico-syntactical rules which govern the use of words and the interrelations of propositions, constitute the subject matter of logic and, in an extended sense, of mathematics. We shall examine them in this and the following chapter, paying special attention to the basic problems of "grounds" and "justification" of inference, and of logical or mathematical "construction." We begin with a discussion of traditional logic.

THE BASIC "LAWS OF THOUGHT"

For purposes of this discussion a general knowledge of traditional or Aristotelian logic is assumed. Our problem is to determine the various elements which provide the foundation for this type of inference and which guarantee its validity.

Inference in all its forms presupposes meaning—although, of course, the specificality of meaning may vary greatly from case to case. At one extreme we find inference dependent upon the interrelation of concretely defined concepts; at the other extreme we deal with abstract symbols or, if you prefer, with universal variables, which have no specific meaning whatever. The inference from "All roses are plants," to "Some roses are plants" is of the first type; the inference "$[(p \supset q) \cdot (q \supset r)] \supset (p \supset r)$" represents the second type. But even this latter type presupposes meaning; for if anything at all is to be inferred from the arrangement of "symbols," each symbol must be appropriately defined: the letters 'p,' 'q,' and 'r,' as representing "propositions"; the symbols '$\supset$' and '$\cdot$' as meaning "implies" and "and," respectively. Meaningless marks on paper, such as these, 묘 | ✵ ☙ ○ ❈ ☛ ❥, provide no basis for inference. Logic, therefore, cannot be reduced to a manipulation of meaningless marks on paper; it is and remains always an operation depending upon and involving meaning.

If this is so, then the most general laws governing the employment of "signs," i.e., the most basic laws governing the definition, fixation, and manipulation of meanings, must be the very laws which provide the broad foundations of all logic.

In an earlier chapter I have shown that the fixation or definition of any particular meaning implies an inescapable commitment to the *law of identity*: A is A. The meaning defined is identical with itself and means what it is defined as meaning. This first law, however, entails a second which, by virtue of this entailment, is equally inescapable, namely, the *law of contradiction*: A is not non-A. The meaning defined is what it is and not what it is not. Lastly, if a meaning has been defined, then any particular object of experience is either part of that meaning or it is not: X is either A or non-A. There is no third alternative. The *law of excluded middle*, in other words, is also inescapably entailed by our commitment to an identifiable meaning.

In brief, intelligibility and meaning are impossible without the three traditional "Laws of Thought"; and in so far as logic presupposes meaning it can escape neither their force nor their scope.

Propositions and Concepts

To the extent to which knowledge is concerned with subject matter, thought is dependent upon concepts and their interrelations. But, as we have seen in Chapter II, concepts are rules for the integration of experience and, as rules, they are inherently propositions. To think of a red rose or a tall man is to think of a rose *as being* red, or of a man *as being* tall, where the word 'being' is but a formal element making more explicit the relation of the subject and the predicate asserted in the original phrase. From such formulations it is a small step to propositions of the type, "This rose is red," or, "This man is tall." The increased formalization transforms the word 'being' into the purely formal copula and changes the respective substantives and adjectives into subject terms and predicate terms of formal propositions. The whole process shows that in thought we effect a severance between substantive and adjective; that we treat them as independently represented; and that we unite them again in the asserted proposition. The proposition as a whole, therefore, is *a unit of meaning formally explicated*; and the concept is a proposition *in nuce*.

What is a concept at one level of reasoning may well be a proposition at some other level. It all depends on how complex the units of

thought are with which we are concerned. The two propositions, "This red rose is beautiful," and, "This rose is red," both concern "this red rose"; but the unit of thought which is taken as in itself complete and which is explicated in the proposition, "This rose is red," is but a conceptualized element in the more complex unit of thought expressed by the proposition, "This red rose is beautiful."

The Primary Relations of Propositions

Once we explicate units of meaning in the form of propositions we discover that each proposition is specifically related to various other propositions, and that its relation to these propositions is such that the truth (or falsity) of any one of them affects the truth (or falsity) of all others in a definite and determinable way. The relations here involved we shall call the *primary relations of propositions.*

Before discussing these relations themselves let us agree upon certain formal matters which will facilitate both presentation and communication.

In the discussions which follow I shall assume that the meaning of the word '*equivalence*' (symbolically expressed by $\equiv$) is understood, and that it is taken as ultimate. I shall assume, furthermore, that the meaning of the word 'not' (symbolically represented by $\sim$) is likewise understood, and it, too, is accepted as ultimate. Lastly, I shall use small letters of the alphabet, p, q, r, . . . , as variables representing propositions.

Let us now consider one of the formal relations of propositions—the relation of *conjunction.* It will be symbolized by the form-word, 'and.' This conjunctive 'and' must, of course, be distinguished from the purely enumerative 'and' of ordinary discourse. When we merely enumerate propositions, each enumerated statement retains its logical independence and may be true or false regardless of the truth or falsity of the other enumerated propositions. But when we conjoin two or more propositions, they fuse into one unitary meaning so that only the conjunctive product as a whole is either true or false. The falsity of any one of the conjoined "elements" implies the falsity of the conjunctive whole.

The relationship expressed by the conjunctive 'and' is subject to at least three additional laws: (1) the "reiterative law": p and p $\equiv$ p; (2) the "commutative law": p and q $\equiv$ q and p; (3) the "associative law": (p and q) and r $\equiv$ p and (q and r). These laws may seem trivial and void of philosophical significance, but they must be accepted

if thought is to advance at all from one proposition to another.

The first law stipulates that the meaning or content of a proposition is unaffected by a reassertion of that proposition. The second law states that the meaning of a proposition is independent of the order among propositions. And the third law asserts that the content of a proposition is indifferent to any specific grouping of propositions.

Now, if two propositions (p, q) and their negations ($\sim$p, $\sim$q) are considered, four specific conjunctive combinations are possible: (1) p and q; (2) p and $\sim$q; (3) $\sim$p and q; and (4) $\sim$p and $\sim$q. If each of these four conjunctions is negated, the resultant statements are the logical equivalents of four other types of compound propositions, namely: (1) $\sim$(p and q) $\equiv$ not both p and q (the disjunctive proposition); (2) $\sim$(p and $\sim$q) $\equiv$ if p then q (the direct implicative proposition); (3) $\sim$($\sim$p and q) $\equiv$ if q then p (the counter-implicative proposition); (4) $\sim$($\sim$p and $\sim$q) $\equiv$ either p or q (the alternative proposition).

Furthermore, each of the non-conjunctive composite propositions can be expressed equally well in any of the three other forms. We thus obtain the following equivalences:

If p then q $\equiv$ If $\sim$q then $\sim$p $\equiv$ Not both p and $\sim$q $\equiv$ Either $\sim$p or q.
If p then $\sim$q $\equiv$ If q then $\sim$p $\equiv$ Not both p and q $\equiv$ Either $\sim$p or $\sim$q.
If $\sim$p then q $\equiv$ If $\sim$q then p $\equiv$ Not both $\sim$p and $\sim$q $\equiv$ Either p or q.
If $\sim$p then $\sim$q $\equiv$ If q then p $\equiv$ Not both $\sim$p and q $\equiv$ Either p or $\sim$q.

If one of these propositions is true, then all propositions equivalent to it are also true.

Still other primary relations among propositions become evident when we examine categorical propositions and their immediate implications. Making use of an additional principle, namely, the principle of "double negation," according to which a double negation is equivalent to an affirmation, we can "obvert" any given proposition and still preserve the identity of its meaning. "All S is P" now becomes "No S is non-P"; "No S is P" becomes "All S is non-P"; "Some S is P" becomes "Some S is-not non-P"; and "Some S is-not P" becomes "Some S is non-P." If the given proposition is true, its obverse is also true.

Propositions may also be "converted"; that is to say, their terms may be interchanged without a change in the quality of the proposition as a whole. An equivalence of meaning is again preserved. In the case of the *E* and the *I* propositions the process is a "simple conversion"

and is accomplished by means of the commutative law for conjunctives, thus:

E propositions: No S is P ≡ Nothing that is S is P ≡ Nothing is S and P ≡ Nothing is P and S ≡ Nothing that is P is S ≡ No P is S.

I propositions: Some S is P ≡ Something that is S is P ≡ Something is S and P ≡ Something is P and S ≡ Something that is P is S ≡ Some P is S.

In the case of the *A* and the *O* propositions conversion cannot be carried out in this simple manner. In both types of propositions S and P differ in their respective extensions, and this difference interrupts the simple chain of equivalences. In the case of the *O* proposition the interruption is irreparable and, in consequence, an *O* proposition cannot be converted. In the case of the *A* proposition we can overcome the difficulty by a specific limitation of the scope of the proposition. The chain of transformation then becomes this:

All S is P ≡ Everything that is S is P (≡) Some things are S and P ≡ Some things are P and S ≡ Something that is P is S ≡ Some P is S.

This transition is possible only if we regard the equivalence indicated by (≡) as acceptable.

Other transitions are not subject to such ambiguities. The *A* and the *O* propositions, for example, may become parts of the following chains:

A propositions: All S is P ≡ Everything that is S is P ≡ Nothing that is S is non-P ≡ Nothing is S and non-P ≡ Nothing is non-P and S ≡ Nothing that is non-P is S ≡ Everything that is non-P is non-S ≡ All non-P is non-S.

O propositions: Some S is-not P ≡ Not everything that is S is P ≡ Something that is S is non-P ≡ Something is S and non-P ≡ Something is non-P and S ≡ Something that is non-P is S ≡ Not everything that is non-P is non-S ≡ Some non-P is non-S.

In both cases we have obtained the "contraposition" of the original proposition. The truth or falsity of one of the constituent propositions is the truth or falsity of the whole chain.

All chains of propositions so far considered presuppose, in addition to the three "Laws of Thought," only two basic and undefined logical ideas—the idea of negation and the idea of equivalence—and two laws or principles of logic—the commutative law and the principle of double negation. But, of course, these chains of "reasoning" also lead to

nothing but equivalent statements. They do not establish conclusions which say more than does the first premise. Immediate inference of this type is a part of logic, but only a part; and it is not even the most important part.

Syllogistic Reasoning

The chains of equivalences referred to in the preceding section represent different ways of expressing identical meanings, and nothing more. All changes and transformations are but formal changes of some given proposition, p. Reasoning, however, if it is to constitute an advance in knowledge, must involve transitions from a proposition, p, to some other proposition, q, which is more than a purely formal transformation of p. Transitions of this type are encountered in all forms of mediated inference, i.e., they are encountered in syllogistic reasoning.

What concerns us here is, of course, not the structure of the syllogism but the ground for its validity. Our questions are, What is the basis of syllogistic inference? What provides the warranty of its compelling necessity? What, in other words, assures us of its unfailing soundness and complete reliability?

The answers to these questions depend upon the very meaning of logic, and entail of necessity an evaluation of various aspects of contemporary "logics" and of logical theory.

Entailment

Syllogistic reasoning, in its most rigid form, depends upon the relation of "*entailment.*" What this means will become clear when we examine a specific example. Consider, therefore, the following argument:

If anything is a cat, then it is a carnivorous mammal; and if anything is a carnivorous mammal, then it is a vertebrate.
Therefore, if anything is a cat, then it is a vertebrate.

In this "hypothetical syllogism" the form-words "if-then" express the relation of entailment. If we examine this relation more closely, we observe that it consists of an indispensable interrelation of meanings. More particularly, we observe that the "antecedent" *entails* the "consequent" simply because the meaning of the "consequent" is already part of the meaning of the "antecedent." The "hypothetical" proposition asserting this entailment merely explicates what is im-

plicitly present in the key term of the "antecedent." This fact requires no elaboration in so far as the two premises of the argument are concerned; for in the case of the propositions which serve as premises the entailment carries with it the cogency and logical compulsion of all "analytical" propositions.

Acceptance of the conclusion of the argument, i.e., acceptance of the proposition, "If anything is a cat, then it is a vertebrate," requires, however, the additional acknowledgment that the relation of "entailment" is *transitive*. If the transitivity of the entailment relation is acknowledged—and in view of the meaning of 'entailment' here assumed such acknowledgment can hardly be refused—the validity of the argument as a whole is no mystery. Its logical necessity follows directly from the meaning of the terms involved.

Restated in augmented form, the argument is actually this:

If anything is a cat, then it is a carnivorous mammal (for the meaning of 'mammal' is an indispensable part of the meaning of 'cat');
and if anything is a carnivorous mammal, then it is a vertebrate (for the meaning of 'vertebrate' is an indispensable part of the meaning of 'carnivorous mammal').
Therefore, if anything is a cat, then it is a vertebrate (for the meaning of 'vertebrate' is an indispensable part of the meaning of 'carnivorous mammal' which, in turn, is an indispensable part of the meaning of 'cat').

Since all "hypothetical" propositions have their "categorical" equivalents, the argument under consideration can readily be translated into categorical form. It is then as follows:

All cats are carnivorous mammals.
All carnivorous mammals are vertebrates.
Therefore, all cats are vertebrates.

Analysis of the propositions here involved will again reveal the entailment relation and its compelling force, and will disclose the same reasons for the validity of the argument as before. That is to say, pure hypothetical arguments and their categorical equivalents, in so far as they involve analytical propositions and nothing else, are *valid because the meanings of terms are what they are.* And this is what we mean by asserting that in these arguments the premises *entail* the conclusion. Entailment, therefore, is not a matter of propositional form as such; *its real ground is the meaning of the terms.*

It follows from this that arguments depending upon entailment

cannot be stated properly in terms of symbols which fail to express the differentiated meanings of terms. It should therefore be no surprise to us that the various propositional calculi of modern "symbolic" logic do not deal adequately with the logic of entailment.

The Applicative Principle

It is true, of course, that the logic of entailment is very much limited in scope. It provides no basis of inference for the vast number of empirical propositions; for these propositions are "synthetical" rather than "analytical," and their truth does not depend upon the definitional interrelation of their terms. Empirical propositions, however, embody the subject matter of the natural sciences as well as the "topics" of our daily discourse. They provide, in other words, the substance of the major part of all our knowledge. A logic is therefore woefully inadequate if it cannot be applied to empirical propositions.

A first step in the broadening of the scope of logic is the introduction of a principle which supplements the law of entailment and is as compelling as the latter. Such a supplementary "law" is what I shall call the *applicative principle of inference.* This principle stipulates that *what is true universally,* i.e., *what is true of each and every member of a given class, is true of any given member of that class.* If, for example, all roses are plants, then this rose is a plant; and if no squares are round, then some squares are not round.

Since the applicative principle itself is quite obviously an analytical proposition, we need not hesitate to accept it. It is dependable and logically compelling; and it is adequate for our purpose. In conjunction with the principle of entailment it accounts fully for the validity of arguments such as the following:

If anything is a material body, then it is extended in space. This object is a material body.
Therefore, this object is extended in space.

The major premise of this "mixed" hypothetical syllogism is an analytical proposition and expresses therefore a relation of entailment. The minor premise, however, is synthetical. Its function is to *apply* the universally expressed relation of entailment to a specific instance, "this body." The two principles (that of entailment and that of application) taken together and both deriving their logical force from the inherent truth of analytical propositions, provide ample warranty for the compelling necessity of the inference in question.

That both principles are involved in the inference can best be seen when the original form of the argument is augmented in such a way as to make explicit the "applicative" step:

If anything is a material body, then it is extended in space (for 'extension in space' is an indispensable part of the meaning of 'material body');
and if this object is a material body, then it is extended in space (for what is true universally, i.e., what is true of each and every member of a given class, is true of any given member of that class);
and this object is a material body (i.e., this object is a member of the class of "material bodies").
Therefore, this object is extended in space (for what is true of each and every member of the class of "material bodies" must be true of *this* particular member of that class).

A translation of this argument into categorical form reveals that the applicative principle provides the basis for categorical arguments of a certain type just as readily as it does for the "mixed" hypothetical syllogism. If anything, the applicative relation is here even more directly evident:

All material bodies are extended.
This object is a material body.
Therefore, this object is extended.

In one other respect can the usefulness of the applicative principle be demonstrated quite easily. The functional equations so widely employed as "laws" of the physical world—the equations of classical mechanics, of classical gas theories, of classical thermodynamics and classical electrodynamics, to refer only to some of the many "laws of nature"—express in their general form a relation of entailment: $S=f(P)$. If such a law is to be employed in the solution of a concrete problem, the argument is in all essentials a syllogism whose major premise is the law in question and whose minor premise (on the basis of the applicative principle) provides specific values for P. The inference itself leads to a conclusion giving some specific value for S. If, for example, the "law" specifies that $s=\frac{1}{2}at^2$—when 's' stands for 'spatial distance passed over,' 'a' stands for 'acceleration,' and 't' stands for 'time'—then the quantitative value of 's' is entailed by the right-hand side of the equation. In other words, the "law" stipulates a relation of entailment. If the minor premise now asserts that, in the particular case under examination, 'a' has the value of 8 cm. per second per second, and 't' has the value of 5 seconds—that is to say,

if the minor premise *applies* the law to specific conditions—then, in this particular case, the value of 's' is $\frac{1}{2} \times 8 \times 5 \times 5$ or 100 cm. In more rigid form:

If anything has values of the form $\frac{1}{2}at^2$, then it has some specific value for s (entailment).
If the instance of $\frac{1}{2} \times 8 \times 25$ has values of the form $\frac{1}{2}at^2$, then it has some specific value for s (application).
The instance of $\frac{1}{2} \times 8 \times 25$ has values of the form $\frac{1}{2}at^2$.
Therefore, the instance of $\frac{1}{2} \times 8 \times 25$ has some specific value for s, namely, 100.

$$s = \tfrac{1}{2}at^2$$
In this case:
$$s = \tfrac{1}{2} \times 8 \times 25$$
Therefore, in this case:
$$s = 100.$$

The form of the syllogism and the principles of entailment and application, taken together, are thus sufficient warranty for the validity of the inference here drawn. The case, however, is typical; and once this fact is admitted, then the broad field of mathematics and of the "laws of nature" has in principle been subsumed under the laws of the syllogism.

Implication

There are, however, arguments generally regarded as valid, which involve still other considerations. The following syllogism illustrates the type:

If the great nations of the world are suspicious of one another, then war is inevitable;
and the great nations of the world are suspicious of one another.
Therefore, war is inevitable.

In the purely formal sense of traditional logic this argument is valid; for it is the well-known form of "affirming the antecedent." It, nevertheless, does not have the compelling force which stems from a relation of entailment; for the major premise is synthetical rather than analytical, and its antecedent does not in itself compel the acceptance of the consequent. That is to say, the truth of the major premise may be challenged successfully. The antecedent as stated is perfectly compatible not only with the given consequent but also with a consequent which asserts that "peace can be preserved by removing all causes or

conditions which give rise to the suspicions." The assertion of one rather than the other of these conflicting "consequents" depends upon matters extraneous to the syllogism itself. The argument, therefore, is compelling only on the condition that the major premise, as stated, is accepted as true. To put it still differently, the soundness of the argument depends upon the truth-value of its major premise; and this truth-value depends upon empirical considerations, not upon the interrelation of meanings.

Whenever the compelling force of an argument depends thus upon an acknowledgment of a specific truth-value, the inference is based upon a relation which, strictly speaking, differs from the relation of entailment. I shall call it the implicative relation or, more briefly, *implication.*

Since all arguments involving the relation of entailment depend upon analytical premises which, by virtue of their "analyticity," are also true, it is perfectly possible to interpret these arguments in terms of implication. But since arguments based upon the implicative relation assume only the empirical truth of their premises, the arguments cannot be interpreted in terms of entailment. After all, entailment implies analytical necessity, while implication implies only a compulsion depending upon an empirical truth. Entailment supplies the inescapable force of definitionally interrelated meanings, while implication supplies only the much weaker force of a validity depending upon an assumed truth-value.

The implicative relation itself, however, may be interpreted in at least two widely different ways: as "material" implication (Whitehead, Russell, and Wittgenstein), and as "strict" implication (C. I. Lewis). Both of these interpretations must now be considered in greater detail.

Empiricism, Formalism, and the Problem of Necessity

The issue we face is, of course, broader than any particular interpretation of the meaning of implication. It involves the whole problem of *necessity* and touches upon the very essence of logic.[1]

So long as we deal with analytical propositions exclusively, the question of necessity creates no difficulties; for the relation of entailment is one of compelling and inescapable necessity, and is such simply by virtue of the definitional interrelation of meanings. But is there a compelling necessity in the implicative relation of empirical propo-

[1] Cf. B. Blanshard, *The Nature of Thought,* II, Chapter XXVIII.

sitions, or can such a necessity ever be established? The first part of this question has already been answered in the negative. The second part requires additional consideration. Is it possible, for example, to establish such necessity on purely empirical grounds?

Suppose I observe that when I hold a burning candle close to a thermometer, the mercury column of the thermometer rises. When I withdraw the candle, the mercury column gradually drops back to its former level. When I bring the burning candle once more close to the thermometer, the column rises again. Upon withdrawal of the candle, I observe again the drop in the height of the column. When I repeat the "experiment" with the candle a third, a fourth, a fifth, and a sixth time, I always observe the corresponding changes in the height of the column of mercury. In my experience, A (burning candle) becomes definitely associated with B (rise of mercury), and C (withdrawal of candle) becomes associated with D (fall of mercury). Additional repetitions of the "experiment" reveal the same associations. And upon the basis of my experiences with this "experiment" I now anticipate that whenever I hold a burning candle close to a thermometer, the mercury column will rise; and that whenever I withdraw the candle, the column will drop. But in anticipating these results I am assuming that whatever happens under certain conditions will always happen under similar conditions. I assume, in other words, that "nature is uniform." The question is, Is this principle of the uniformity of nature justifiable *on empirical grounds*? If it is, then empiricism may provide an adequate basis for the compelling necessity of inference; but if it is not, then empiricism precludes the idea of necessity and can furnish no dependable foundation for logic.

If the empiricist asserts the principle of the uniformity of nature as a truth requiring no proof, he is, of course, inconsistent in his own position; i.e., he violates the principle of empiricism itself. But when he attempts to prove, on empirical grounds, that nature *is* uniform, his argument either fails to yield the desired conclusion or it involves a *petitio*. After all, the principle of the uniformity of nature is universal in scope; but the empiricist's appeal to the "facts" of experience can be an appeal to particulars only. At best he points to a variety of "uniform sequences" which have come under his observation—"uniformities" such as these: when sufficient heat is applied to a container filled with water, the water begins to boil; when water boils, steam is generated; when a seed is placed in fertile soil, it will grow; when eggs are kept for some time at a certain temperature, they will

hatch. The principle of uniformity, he must argue, is but an interpolation or extension of these observed uniformities.

But how does the empiricist know that the observed sequences really are uniform—uniform, that is, beyond the range of his actual observation? Compared to all possible happenings, he and his fellow empiricists together can actually observe only an infinitesimal segment of each sequence and only an exceedingly small number of all possible sequences. But only a complete enumeration of all instances can ever furnish an "empirical" proof of a universal; and, in the case of the principle of uniformity, the restrictions imposed by time and space and the limitations of opportunity are insurmountable for the observer. If, nevertheless, he is confident that the fragmentary "uniformities" which he has observed bespeak a general "uniformity of nature," his confidence is warranted not by his actual observations, but by his implicit assumption of the very principle the truth of which he intends to prove.

But does not the scientist do exactly what the empiricist tries to do? And does he not do so without encountering the charge of indulging in a question-begging argument? From the observation of a few instances of some particular "uniformity," the scientist "derives" a law which purports to hold for all instances of that "uniformity." When Galileo formulated the law of falling bodies, he formulated a universal law, i.e., he formulated a law pertaining to *all* cases of falling bodies; and he formulated this law without having actually observed all possible cases. The law, nevertheless, is accepted as universally valid. It expresses a necessity which is compelling and inescapable.

Two comments are in order: (1) Galileo's law of falling bodies (or any other scientific law) implicitly assumes that nature is uniform. It assumes, in other words, the truth of the principle of uniformity and would lose its compelling force should that principle turn out to be false. (2) The case of the empiricist in epistemology is not analogous to that of the scientist in his special field. The latter is not concerned with the ultimate principles upon which cognition rests; he can and does presuppose the validity of these principles and can and does proceed at once with his more specialized task of formulating specific laws which derive their universality from the principles assumed to be true. There is no danger in this so long as the logical ground upon which scientific laws rest is clearly understood. The epistemological empiricist, however, must deal with that logical ground itself; i.e., he must furnish an acceptable justification of the very

principles which the scientist can assume as true. And it is in his pursuit of this task that the empiricist must either abandon all argument or involve himself in a *petitio*.

What is true in the case of the principle of uniformity is true also when the laws of logic are at issue. If anything, the difficulties of the empiricist are here even greater. Not only do all his efforts at positive proof fail because of an inescapable *petitio*, but the very denial of the universal validity of the laws of logic, if it is supported at all by argument, assumes the validity of those laws and thus frustrates all attempts at escape.

It is impossible to justify the laws of logic by deriving them from something "more ultimate"; for these laws are themselves the warranty of intelligible thought and of inference. And any proof which might demonstrate that the laws of logic are "mere habits" of thought and that they entail no compelling necessity, is itself compelling only to the extent to which it assumes and involves the unquestionable and universal validity of the disputed laws. Whenever the empiricist admits the compelling force of an argument, he is confronted with a type of necessity—the compelling necessity of logical inference—which cannot be explained on empirical grounds. The dilemma in which he finds himself is decisive; for if logical arguments are valid, the empiricist cannot account for their validity without abandoning his position; and if logical arguments are not valid, the empiricist cannot establish his case by argument.

Formalism and Necessity

The difficulties of the consistent empiricist arise, in the last analysis, from the fact that he regards sensory qualia *and nothing else* as the basis and source of all knowledge. For him, as for Hume, nothing can be in the mind that was not first in the senses. Necessity, however, is not a sense quale. It is not "given" through the senses. We cannot see it; nor can we hear, taste, or touch it.

A more moderate empiricist may admit all this, but may point out at the same time that all sense qualia are experienced by us in various groupings or "arrangements"; that they are not isolated data but *structural complexes*, and that they form certain "configurations" or "patterns" which endure and which are duplicated or which recur in various combinations. The sensory configurations of first-person experience, he may argue, as patterned groupings, involve elements of *form*, which, though not directly perceived through the senses, per-

meate the whole of experience. And if he is a "logical" empiricist, he may maintain that these elements of form provide the basis for whatever necessity can be found in the laws of thought and in logical arguments.

There is danger, of course, that the legitimate distinction between "form" and "content" may lead to a complete separation of these two aspects of experience. There is danger, in other words, that the "logical" empiricist will drift into the position typified by Moritz Schlick's statement that "nothing of the content of our enormously manifold experiences can be made the object of a proposition, and (that) therefore propositions can have no meaning save that which their purely formal relations express" (Schlick, p. 149).

That this extreme *formalism* is an untenable position can readily be seen when we examine more closely the actual interrelations of "form" and "content." Nowhere in our experience does "form" ever occur in isolation or without content; but everywhere do we find that "form" and "content" are relative matters. What is "form" at one level of experience is "content" at some other level. The simpler "forms" are "content" of the more complex patterns. A bouquet, for example, is the "form" of its component flowers. Each flower is the form of its specific stems, leaves, and blossoms. And each blossom, in turn, is the form of its own petals, stamens, and pollens.

Furthermore, do propositions really "have *no* meaning save that which their purely formal relations express"? Consider for a moment the proposition, "No circles are rectangular." If we disregard the specific "content" of this proposition, i.e., if we disregard the specific meanings of 'circle' and 'square,' the statement simply asserts that no member of a subject-class possesses qualities designated by some predicate term. But is this all that the proposition asserts? If it is, then a substitution of specific terms can make no difference in its meaning so long as we retain a subject-class and a predicate term. An actual substitution, however, proves disastrous to such complacency; for, surely, the proposition, "No squares are rectangular," is of the same "form" as is the proposition, "No circles are rectangular"; and yet the truth-values of the two propositions are radically different—one proposition being false, the other true. Clearly, then, the "meaning" of propositions in relation to their truth or their falsity is not simply a matter of "pure form." "Content" is here indispensable.

Moreover, as has already been shown in a previous section, the

"purely formal" statement of propositions—be it symbolized by "All S is P," "$p \supset q$," "$(x)\ \psi_x \supset \phi_x$," or by any other device—is actually *not* "pure" form; for the arrangements of such symbols could not even be regarded as "propositions" (or as "propositional functions") were it not for the fact that they are defined in specific ways, i.e., were they not associated by definition with certain "contents" of experience rather than with others. The sequence of symbols "$(x)\ \psi_x \supset \phi_x$," for example, has logical significance only if it is understood (1) that '(x)' means "for all cases of x"; (2) that 'x' designates "some particular thing or referent"; (3) that 'ψ' and 'ϕ' signify different "attributes"; and (4) that '$\supset$' has the meaning of "implies." Without this (or a comparable) "interpretation" the sequence of "symbols" as given above is literally meaningless and therefore not even a sequence of *symbols*. And if it is meaningless, it is irrelevant to logic (cf. Cohen).

If "pure form," i.e., form devoid of all content, is thus a fictitious abstraction, it seems evident that whatever necessity we find in the principles and laws of logic cannot stem from form alone. A reference to content is indispensable; and, as a matter of fact, there is much evidence to support the contention that it is *primarily* the content of propositions which entails necessity and logical compulsion.[2]

Whenever analytical propositions are involved, this reference to "content" is obvious. The compelling necessity which we encounter in the relation of entailment derives from the definitional interrelation of meanings: i.e., it derives from "content." But what about propositions such as this: "Red differs from orange less than from yellow"? This proposition obviously asserts (1) that "red" differs from "orange"; (2) that "red" differs from "yellow"; and (3) that the "difference" between red and orange differs from the "difference" between red and yellow. The third point, the assertion of a *difference of differences*, i.e., the assertion of a *necessary distinction within the range of a "formal" relation*, involves the crucial issue; for this distinction cannot be made on formal grounds alone. It is understandable only when we consider the terms involved in their relation to the specific sense qualia which they designate. If we have the colors (red, orange, and yellow) directly before us, we "see" that the difference between red and orange differs from the difference between red and yellow; we "see" the necessity of distinguishing between various "differences." And when we express this difference of the two "differ-

[2] Cf. B. Blanshard, *The Nature of Thought*, II, Chapter XXIX.

ences" in the proposition, we do so not out of some necessity imposed upon us by form, but out of the necessity to state adequately the content of experience. "Content" very definitely determines here the structure or form of the proposition.

But consider also propositions such as these: (1) "The volume of a sphere equals $4/3\,\pi\, r^3$"; (2) "If A is contemporaneous with B, and if B is later than C, then A is later than C"; (3) "$2 \times 6 = 3 \times 4$"; and (4) "$a^2 - b^2 = (a+b)(a-b)$." Does the compelling nature of these propositions lie in their "content" or in their "form"?

To be sure, proposition (1) is true regardless of any particular sphere to which it may be applied. Proposition (2) holds true no matter what 'A,' 'B,' and 'C' stand for. Proposition (3) is compelling regardless of what "things" are represented by '2,' '3,' '4,' and '6.' And proposition (4) compels acceptance irrespective of any particular numbers which may be substituted for 'a' and 'b.' But does this mean that these propositions compel acceptance because of their form only?

If content had no bearing upon the "necessities" which these propositions express, then various interchanges or substitutions of "content" should leave their compelling force unaffected. It can easily be demonstrated, however, that this force does not remain unaffected when changes of content are made. Proposition (1), for example, is manifestly true only so long as we deal with the "volume of a sphere." If we substitute the "area of a sphere" or the "volume of a cube" for "volume of a sphere," the proposition not only loses its compelling necessity, it ceases to be true. The necessity expressed in proposition (2) stems not exclusively from the form of that proposition but in part at least also from the peculiar nature of time. And while it is true that some other relations entail a corresponding necessity—e.g., "If A is equal in weight to B, and if B weighs less than C, then A weighs less than C"—relations cannot be substituted indiscriminately for those actually given in the proposition. The following proposition, for instance, although of exactly the same form as (2), can hardly be regarded as true: "If A is taller than B, and B weighs less than C, then A weighs less than C." The "content" here has its unmistakable bearing upon the "necessity" expressed in the proposition. In a similar way, proposition (3) is true only when '2,' '3,' '4,' and '6' are appropriately interpreted as specific "quantities." "$2 \times 6 = 3 \times 4$" is hardly true if '2' means "blue," '$\times$' means "number," '6' means "truthful," '=' means "yesterday," '3' means "electron," and '4'

means "poem." Proposition (4) becomes equally meaningless when such terms as "virtue" or "tree" are substituted for 'a' and 'b.'

The "form" of the syllogism, finally, depends ultimately also upon a reference to "content" and, therefore, not upon "form" alone; for if an argument is to be acceptable as valid, its terms must have *the same meaning* throughout. This appeal to an identity of meaning is, in the last analysis, a reference to "content" rather than to "form"; and whether or not it has been heeded in any given case can be determined only by examining the "content." The symbolic representation of the syllogism obscures the facts by assuming implicitly that each term retains an identity of meaning throughout the argument. Actually no complete identity of the terms is preserved in any syllogism. Even in the schematic form of

All M is P
All S is M
Therefore, all S is P,

'M' in relation to 'P' is "species," while in relation to 'S' it is "genus." 'P' in relation to 'S' is of the specific nature of which 'SP' is true, whereas in relation to 'M' it is such that 'MP' is true. To assume that 'P' undergoes no alteration of meaning whatever through these different associations is to assume that *all* relations of terms are external; and this assumption is untenable.

Even though all whales are mammals and all cats are mammals, whales are mammals of a distinctive kind; and their kind differs from the kind of mammals cats are. Cats and whales, in other words, are both mammals, but the being-a-mammal of the whale (in its structure and mode of living) is different from the being-a-mammal of the cat (in its structure and mode of living). Only the abstractionism implied in the assumption that all relations are external can lead to a denial of the specific differentiations of 'mammalian nature,' depending upon whether it is manifested in whales or in cats or in some other particular "species." The "mammal in general" belongs to the limbo of "abstract universals."

The point I am trying to make becomes perhaps more obvious when the following syllogism is examined:

All advocates of a free society work for a return to the principles of *laissez-faire.*

All advocates of a free society desire to control industrial production and the distribution of goods in the interest of society as a whole.

Therefore, at least some persons who desire to control industrial production and the distribution of goods in the interest of society as a whole work for a return to the principles of *laissez-faire*.

The fallacy of "four terms" here involved becomes evident from the very relation of 'M' to 'P' and to 'S'; i.e., it becomes evident from the meanings of 'MP' and 'MS,' respectively. But where is the line of demarcation which separates the *identity* in meaning of 'M' from the *ambiguity* of 'M' entailing a fallacy of "four terms"? In discussing "democracy" or "Christianity," for example, when do we still speak about the same thing, and when do we no longer do so?

My argument throughout this section is not that necessity depends exclusively upon "content," but rather that it does not depend upon "form" alone. In critical matters "content" and "form" must be considered together, and only their combination can provide an adequate basis for logical necessity. Only their conjunction is adequate to the nature of thought *as thought is employed in the achievement of knowledge*. My contention is, furthermore, that even symbolic logic, *as logic*, is not entirely without "content," and that logical calculi, as *logical* calculi, are by no means a matter of "purely formal" relations of "meaningless" symbols.

"Material" Implication

In order to clarify further the contention just stated and to examine at the same time from a still different angle the problem of logical necessity, I turn now to a consideration of symbolic logic as first developed by Whitehead and Russell.

The idea of "implication," as defined in *Principia Mathematica*, is of particular interest in this connection; for it is at once the crucial relation in the calculus developed in *Principia*, and the crucial problem pertaining to necessity and logical compulsion.

'Implication,' according to Whitehead and Russell, designates a relation of propositions of such nature that it is *false in fact* that a true "antecedent" can have a false "consequent." If p "implies" q, then either p is false or q is true ($p \supset q = {\sim}p \vee q$), and it is *false in fact* that p is true and q is false. The phrase "false in fact" is all-important.

We can best understand the true nature of this "implicative relation" when we compare it with the relation of entailment as previously defined. Entailment, as will be remembered, is entirely a matter of interrelated and interdependent meanings. It is encountered whenever

one term designates something which is part of the meaning connoted by some other term. The factual truth or falsity of the propositions of which these terms are constituent elements is irrelevant to the relation of entailment. It is otherwise in the case of "implication" as defined by Whitehead and Russell. The "implicative relation" of *Principia Mathematica* depends not upon the interrelations of meanings but upon the asserted truth or falsity of the propositions in question. And it depends upon this truth or falsity *regardless of what each proposition denotes.*

If 'p' and 'q' designate propositions, then, with respect to their truth or falsity, four combinations are possible. Either (1) both propositions are true (p and q); or (2) both propositions are false ($\sim$p and $\sim$q); or (3) the first proposition is false and the second true ($\sim$p and q); or (4) the first proposition is true and the second false (p and $\sim$q). Whenever one of the first three combinations is encountered, then, according to Whitehead and Russell, p "implies" q. That is to say, the relation of "implication" is such that it precludes only combination (4). p always implies q except when p is true and q is false. "Implication" so defined is called "material" implication.

At once two important consequences of this definition are apparent: (a) It follows from combinations (2) and (3) that a false proposition implies *any* proposition, be it true or false. (b) It follows from (1) and (3) that a true proposition is implied by *any* proposition, be it true or false. Does this situation express adequately the meaning of logical necessity?

Consider the following propositions: "All birds have wings," and "All trees have roots." Both propositions are true. And since this is the case, the first "materially implies" the second; i.e., it is true that "*if* all birds have wings, *then* all trees have roots." But does the relation here indicated by "if . . . then" actually compel acceptance? Does the "consequent" really *follow from* the "antecedent"? Could we *infer* the "consequent" if only the "antecedent" were given? The answer to all of these questions is an unqualified No. There is nothing compelling in the relationship of p and q as exemplified in this case. The "consequent" does *not* follow *from* the "antecedent" in a logical sense but presupposes independent assertion.[3]

Consider next the propositions, "All dogs have seven legs," and "All men are mortal." Since, in the sense of "material implication," a false proposition implies *any* proposition, be it true or false, it

[3] Cf. B. Blanshard, *The Nature of Thought*, II, 377-385.

should be true that the false proposition, "All dogs have seven legs," *implies* the true proposition, "All men are mortal." "All men are mortal," in other words, is a "consequent" of "All dogs have seven legs" (or of any other false proposition we can think of). But if this is the meaning of "material implication"—and there can be no doubt that it is—then it is evident that the relation so defined is unacceptable as a basis for logical inference. At best it is a device which enables us to state in the form of an implication that which we have already ascertained prior to, and without the help of, an inference. No proposition can ever be said to be "materially implied" by another until after the truth-values of both are known. In genuine inference, however, we advance from known propositions to propositions not yet known. When we show that a given hypothesis "implies" such and such particular consequences, we do not first prove that these "consequences" possess a certain truth-value and *that in virtue of their truth-value* they are "materially implied" by the hypothesis; we proceed rather by inferring the "consequents" from the hypothesis itself, and we do so irrespective of the respective truth-values. Only after the consequents have been drawn do we try to establish their truth or falsity. If they are true, they "confirm" the hypothesis; if they are false, they "disconfirm" it. But "confirmation" and "disconfirmation" of an hypothesis have no bearing upon the implicative relation as such; and the latter is quite independent of the former.

The relation designated in *Principia* by the symbol '$\supset$' and defined by "$\sim p \vee q$" (i.e., the relation of "material implication"), *when interpreted as implication in any logical sense,* gives rise to a series of paradoxes which reveal most clearly how far removed from the logical meaning of implication the relation of "material implication" really is. For example, since a false proposition "materially implies" *any* proposition, be it true or false, it implies its own truth: $\sim p \supset p$; e.g., the falsity of the proposition "All squares are round" *materially implies* the truth of the proposition "All squares are round." If p does not imply q, then it implies the falsity of q: $[\sim(p \supset q)] \supset (p \supset \sim q)$; e.g., if the proposition "Today is Tuesday" does not imply the proposition "All stars are self-luminous," then it implies that "stars are not self-luminous." And if p does not imply q, then q implies p: $[\sim(p \supset q)] \supset (q \supset p)$; e.g., if the proposition "Grass is green" does not imply the proposition "Honesty is a virtue," then the proposition "Honesty is a virtue" implies that "Grass is green."

Such paradoxes as these—and others can be found—disappear

when '$\supset$' is interpreted, not as signifying an implication, but as designating "$\sim p \vee q$"; for the paradoxes are paradoxes of implication, and if we eliminate the idea of implication we eliminate the "paradoxes" *as paradoxes* (cf. Bronstein). But when we do this, the relation between the "primitive propositions" of *Principia* and the "theorems" presumably deduced from them must also be expressed by '$\sim p \vee q$' and not by an implication; i.e., the relation is then such that either the "primitive propositions" are false or the "theorems" are true. And this means that the "theorems" need not be logical consequences of the "primitive propositions." This consequence of the non-paradoxical interpretation of '$\supset$' is, however, the very denial of the purpose and aim of *Principia*; for the "calculus of propositions" developed by Whitehead and Russell purports to be a strictly deductive system in which the "theorems" follow with logical rigor from the definitions and postulates.

There is thus no escape. Either the "calculus of propositions," in so far as it depends upon the relation designated by '$\supset$' is not a deductive system, or it is a system of "material implication" and yields paradoxes which are embarrassing from the point of view of logic. Whichever alternative we choose, we have no adequate statement of logical implication or of logical necessity.

This does not mean that the calculus of propositions, as developed by Whitehead and Russell, is without merit. If we start with the relation of entailment and adhere to it rigorously, we can employ the symbolism of the calculus and can thus profit from the clarity and simplicity of its expressions. But if we accept the calculus and adhere to its definition of "material implication," we find it impossible to translate every expression or argument of the calculus into a compelling sequence of meanings. "Material implication" permits the combination of propositions in an "if . . . then" form even when no implicative connection in any *logical* sense, i.e., in any sense of related meanings, can be discerned. To the extent, then, to which logic involves specific meanings, the Whitehead-Russell "calculus of propositions" is not logic; and it certainly is not *the* logic (cf. Nelson).

"Strict" Implication

A more rigorous interpretation of implication has been furnished by C. I. Lewis in his system of "strict" implication. Lewis concedes that "material" implication does not provide an adequate basis for inference, that, in fact, it does not in the least resemble what we

ordinarily mean by 'implication.' He believes, however, that this deficiency of the calculus of propositions can be overcome through a more careful definition of the relation in question.

Assuming a set of "primitive ideas" such that '$\Diamond p$' means "p is possible," and 'o' means "is consistent with," Lewis defines the possibility of p being true in terms of the self-consistency of p:

$$\Diamond p = p \circ p,$$

and the possibility of p and q both being true in terms of the mutual consistency of p and q:

$$\Diamond pq = p \circ q$$

He then defines *implication* as follows:

$$p \mathrel{-\!\!\!3} q = \sim\Diamond(p \sim q),$$

i.e., p *strictly implies* q when it is *impossible* for p to be true and q false. From this definition (in conjunction with the idea of consistency) the following proposition can be derived—a proposition which clearly states the relation between "strict" implication and logical consistency which characterizes Lewis's system:

$$p \Upsilon q = \sim(p \circ \sim q);$$

i.e., 'p strictly implies q' means that p is not consistent with $\sim$q, i.e., that the statement 'p is true and q false' is not self-consistent. To put it in still different words, p strictly implies q when an assertion of p and a denial of q amounts to a self-contradiction.

The difference between "strict" implication and "material" implication is fairly obvious. Whenever p "strictly" implies q, it implies it "materially" also; but not all cases of "material" implication are also cases of "strict" implication. The requirements of "strict" implication are much more rigorous than are those of "material" implication. The fact that two propositions, p and q, happen to be true (which is sufficient ground for asserting that p "materially implies" q) does not in itself warrant the assertion that p "strictly implies" q. p "strictly implies" q only if the affirmation of p and the simultaneous denial of q involve us in a self-contradiction. For example, since the propositions "All roses are plants" and "All horses are mammals" are both true, the proposition "All roses are plants" *materially* implies "All horses are mammals," but it does *not strictly* imply it; for the assertion that "All roses are plants" is not inconsistent with the denial of

"All horses are mammals." "Material" implication is encountered whenever, ***because of their actual truth-value,*** we cannot affirm the "antecedent" and deny the "consequent." "Strict" implication is encountered whenever, ***because of a resultant self-contradiction,*** we cannot affirm the "antecedent" and deny the "consequent."

The question is, Does "strict" implication provide an adequate interpretation of that particular relation between antecedent, p, and consequent, q, which is at once the necessary and the sufficient condition of valid inference? It is, of course, true that ***whenever q can be validly inferred from p,*** a denial of q necessitates a denial of p. To affirm p and deny q would indeed be inconsistent. If an object is red, then it is colored; and its being colored can be validly inferred from the fact that it is red. To assert that the object is red and deny that it is colored means to indulge in a contradiction. Inconsistency, therefore, may well be looked upon as a negative criterion of valid inference (cf. Blanshard). But a ***negative criterion*** of valid inference is ***not*** the same as the ***relation itself*** in which that inference is grounded. We can say at best only that whatever else the relation between antecedent and consequent may be, it is at least such that for any given antecedent the denial of its "implied" consequent results in a contradiction. But is this sufficient as a definition of the relation in question?

That it is not becomes apparent, I believe, when we consider the paradoxes which Lewis's system entails. Consider, for example, the following theorems: $\sim \Diamond p \cdot \strictif \cdot p \strictif q$, 'an impossible proposition implies *any* proposition'; or $\sim \Diamond \sim p \cdot \strictif \cdot q \strictif p$, 'a necessary proposition is implied by *any* proposition.' According to the first "paradox," the impossible proposition "All circles are squares" "strictly" implies "All birds are vertebrates"; and, according to the second "paradox," the necessary proposition "All squares are rectangles" is "strictly" implied by "Snow is white." But does "All circles are squares" imply that "All birds are vertebrates" ***in the same sense*** in which the conjunctive proposition "All animals possessing a segmental spinal column are vertebrates and all birds possess a segmented spinal column" implies "All birds are vertebrates"? Or is the sense in which the proposition "All squares are rectangles" is implied by "Snow is white" ***the same as*** that in which it is implied by "All right-angled parallelograms are rectangles and all squares are right-angled parallelograms"? To raise these questions is to answer them in the negative; for "strict" implication—which admittedly holds whenever logical

inference is possible—also holds in some cases where inference in any logical sense is not possible (cf. the cases just cited).[4]

This conclusion contradicts Lewis's own contention that "the relation of strict implication expresses precisely that relation which holds when valid deduction is possible, and fails to hold when valid deduction is not possible."

It is true, of course, that the "paradoxes" of strict implication are paradoxical only when we identify the calculus in which they appear with logic itself; i.e., if we interpret "strict" implication as pertaining to the interrelations of meanings. So long as our concern is with meanings and their interrelations, the "paradoxes" cannot be allowed as legitimate "theorems" of logic. And this means that from the point of view of intentional logic the definition of "strict" implication must be repudiated as being inadequate.

A final question arises: Why should intensional logic be regarded as the criterion of adequacy in this matter? The answer is that valid deduction depends upon meaning or logical import, that is, it depends upon intension. If two propositions are to be related to each other in such a manner that the second can be validly inferred from the first, then the meaning of the second must be part of the meaning of the first. Thought, dealing with subject matter and being deeply concerned with the advancement of knowledge, can see nothing but self-repudiation and a complete abandonment of its own integrity in the theorem of "strict" implication which stipulates that *any* proposition can be *validly inferred* so long as its "antecedent" is *some impossible* proposition (cf. Blanshard).

We conclude therefore that neither "material" implication nor "strict" implication can be regarded as exact equivalents of that peculiar interrelation of propositions which makes valid deduction possible, and which is adequately defined in terms of entailment and application.

Alternative "Logics"

Neither "material" nor "strict" implication, so we have seen, provides an adequate basis for logical necessity. Both fail to account for the compelling nature of deduction; for they do not define unambiguously the specific relation between propositions upon which valid inference rests.

[4] Cf. B. Blanshard, *The Nature of Thought*, II, 385-398.

We shall now examine a line of development of symbolic logic which leads far beyond problems of "implication"—a line of development, in fact, which includes the construction of various *alternative* and *non-Aristotelian* "logics" (cf. Cunningham, Lewis, Lukasiewicz, Rougier, Weiss). The problem we face is this: Is logic ultimately *one*, or must we concede that there exists an irreducible *plurality* of differently constructed but equally valid logics?

The impetus for this new development in symbolic logic stems from Wittgenstein's *Tractatus Logico-Philosophicus.* The laws of logic, Wittgenstein said, are "compelling" because they are *tautologies.* They are unconditionally necessary because their truth is dependent neither on the meaning nor on the truth-value of the elementary propositions of which they are composed. The assertion that a proposition p expresses the same idea as does a proposition q can be verified only if we examine the meanings of p and q; i.e., the truth of the assertion depends upon meanings. The assertion that "of two propositions, p and q, one is true and the other false," can be verified only if the truth-values of p and q are such that the two propositions are not simultaneously true or simultaneously false; i.e., the truth of the assertion depends on the truth-values of its component parts. The reference to specific truth-values is here as indispensable as is the reference to meaning in the first example. However, the proposition "p is either true or false" is true regardless of the meaning or the actual truth-value of p. It is true, Wittgenstein argues, because, by convention, any proposition p has only two truth-values—namely, true and false—and the proposition "p is either true or false" exhausts all possibilities of truth-values of p. And it is true also regardless of any particular meaning of p. It is true, in other words, because it is a *tautology.*

But the proposition "p is either true or false" is also a "law of logic," namely, the *law of the excluded middle.* It may, therefore, be taken as typical of *all* laws of logic, and its justification as the justification, in principle, of all laws of logic. All of these laws are "tautologies" and can be shown to be such. The formal device for effecting the demonstration is Wittgenstein's well-known "matrix procedure."

Wittgenstein starts with the "conventional" stipulation that any given proposition has two, and only two, truth-values. It is either true or false, and it cannot be true and false at the same time. Hence, if we conjoin two propositions, p and q, four, and only four, different

combinations of truth-values will be obtained. This fact is expressed by the following "reference column" of the matrix:

p	q
T	T
T	F
F	T
F	F

Since each combination may itself be true or false, the following *exhaustive matrix* may be constructed:

p	q	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
T	T	T	T	T	T	T	T	T	T	F	F	F	F	F	F	F	F
T	F	T	T	T	T	F	F	F	F	T	T	T	T	F	F	F	F
F	T	T	T	F	F	T	T	F	F	T	T	F	F	T	T	F	F
F	F	T	F	T	F	T	F	F	T	T	F	T	F	T	F	T	F

When a combination of p and q is *true for all values,* as indicated in column 1, Wittgenstein calls it a *tautology.* "Tautologies," in other words, are combinations of propositions which are "true" regardless of the specific meaning of their component elements and regardless also of the truth or falsity of each component proposition.

When a combination of p and q is *false for all values,* as indicated by column 16, Wittgenstein calls it a *contradiction.* That is to say, "contradictions" are combinations of propositions which are "false" regardless of the truth or falsity of the component propositions, and regardless also of the specific meaning of the component elements.

Column 5, as given above, defines the relation of *implication.*

Employing the principles of the "exhaustive matrix," Wittgenstein shows that the laws of logic are all tautologies and that logical deduction itself is "tautological," involving nothing but a tautological transformation of given propositions.

Several comments are in order:

(1) The "matrix procedure" itself presupposes the validity of at least three basic principles, namely, the validity of the "law of identity," the validity of the "law of contradiction," and the validity of the "law of the excluded middle." Even though these laws may be "derivable" by means of the matrix, such "derivation" is of only secondary interest because it is itself possible only if the validity of the laws in question is implicitly assumed in the "derivation." Consider the following matrix:

p	q	$\sim p$	$\sim q$	$\sim p . \sim q$	$p \vee q$	$(\sim p . \sim q) \vee (p \vee q)$
T	T	F	F	F	T	T
T	F	F	T	F	T	T
F	T	T	F	F	T	T
F	F	T	T	T	F	T

It is evident that the stipulation of two and only two truth-values asserts what is in effect the "law of the excluded middle." This same law underlies the derivation of the truth-values of $\sim p$, $\sim q$, and of p and q in all but the reference columns. But the principles of identity and of contradiction are also involved. The fact that T, F, p, q, etc., retain their original meaning regardless of their position in the matrix is due to the implicit assumption of the law of identity; for change in position *could* entail a change in meaning—as may be seen from our usage of writing numbers by position.

Furthermore, the tautology $(\sim p . \sim q) v (pvq)$ is obtained by advancing step by step from the left-hand column (the reference column) to the right-hand column of the matrix; and this advance is possible only because of a specific integrational relation of the columns in question. In other words, it is possible only because the matrix as such is a *synthetic whole* and not a tautology. The matrix procedure itself is not a tautological transformation of propositions but verifies Kant's contention that "synthesis is the first thing which we have to consider, if we want to form an opinion on the first origin of our knowledge."

Our findings are not altered by the fact that the "tautological character" of a "proposition of logic" can be recognized by analyzing it in isolation; for the principles of identity, of contradiction, and of the excluded middle, as well as certain synthetic relations (such as the rules of syllogistic inference), are always implicitly assumed in such a procedure. Were meanings and the interrelations of meanings not what they are, even the construction and intelligible employment of matrices would be impossible.

(2) An examination of column 5 of the "exhaustive matrix" reveals that "implication" is here identified with "material" implication. The criticisms which culminated in our repudiation of the Whitehead-Russell definition of "implication" are therefore germane in this case also. "Material" implication, defined by matrix procedure or in some other way, is and remains inadequate as a basis for logical deductions.

(3) Wittgenstein's matrix procedure tends strongly toward an

"atomistic" theory of knowledge for it seems to imply that the truth-values of some propositions can be directly ascertained. To the extent to which this is true, the matrix procedure assumes a "correspondence" criterion of truth, if not a basic intuitionism, and is subject to the criticisms directed against all "correspondence" theories and against the idea of self-evident propositions (cf. Chapter IV).

(4) Going beyond Wittgenstein, but in complete harmony with the principles of the matrix procedure, Paul Weiss has argued that there is no a priori reason why implication should be defined exclusively by the combination of p and q as given in column 5 of the "exhaustive matrix." Barring tautologies and contradictions, Weiss maintains, any one of the other possible combinations may serve as a definition of implication, and in each case the result will be a new "logic."

It is, of course, true that we can attach the *name* 'implication' arbitrarily to any one of the combinations of propositions given in the matrix; but to argue that each combination so named is the key concept to a new "logic" is either to play fast and loose with the term "logic," or to indulge in a *non sequitur.*

Consider, for example, the specific case in which the truth-value combination of column 9 (as given above) defines "implication." An inference is then impossible whenever both the antecedent and the consequent are true; but it is possible whenever one or the other is false, or when both are false. The inference, therefore, from "This rose is red" to "This rose is colored" is impossible—by definition of the term 'implication'; whereas the inference "All roses are plants; therefore, no triangles are geometrical figures," is not only possible but valid—again by definition of the term 'implication.' If this is *logic* and not merely some arbitrary play with meaningless marks on paper, then knowledge and rationality have lost all significance, and thought itself perishes in an abyss of absurdities.

(5) Wittgenstein, it will be remembered, maintained that *by convention* a proposition p has only two truth-values—true and false. If this is really *nothing but a convention,* there is no compelling reason why anyone should adhere to it; there is no compelling reason why we should not assign three, four, five, or *any* number of truth-values to a given proposition. As a matter of fact, the matrix procedure supplies the formal device for the development of innumerable *polyvalent* or *n*-valued "logics." It provides, in other words, the mechanism for the development of "alternative logics."

We may stipulate, for example, that any proposition has the "truth-

values" T, P, and F. We can then construct a matrix based upon the following reference column:

T	T
T	P
T	F
P	T
P	P
P	F
F	T
F	P
F	F

Certain combinations of p and q will again turn out to be "tautologies" in Wittgenstein's sense and will thus constitute the laws of a three-value "logic."

If we care to do so, we can construct matrices involving *n* "truth-values," where *n* signifies any finite number or any denumerable infinite number. There is thus no end to the number of possible "logics."

Modifications of such polyvalent schemes are the "modal logics" of Goedel, MacColl, Lukasiewicz, and others. In these "logics" the basic values of 'true' and 'false' are retained, but they are qualified by 'necessary,' 'possible,' 'contingent,' 'impossible,' and so on. Instead of designating a proposition as p (read: "p is true"), we may introduce "modal operators" and write Np, Pp, Ip, which are to be read as: "It is necessary that p is true," "It is possible that p is true," "It is impossible that p is true." "Logical" calculi of three, five, and six "modalities" have already been constructed. But if we modify the "modal propositions" themselves by modalities—that is to say, if we form "modal" assertions of the type, "It is necessary that p is necessarily true," "It is possible that p is necessarily true," "It is impossible that p is necessarily true," etc.—we may be able to go on indefinitely constructing "modal calculi."

But even this is not the end of matrix possibilities. All "logics" so far considered presuppose either the "principle of the excluded middle" or a corresponding "principle of the excluded $(n+1)$." If this principle is discarded, a still different type of "logic" may be developed. Of this type, the "intuitionist logic" of Heyting is a fair example. Its radical difference from "ordinary" logic becomes apparent when we compare the basic principles of the respective calculi.

"Affirmation," for instance, is now no longer equivalent to "double negation"; for in Heyting's calculus affirmation still implies double negation, but double negation no longer implies affirmation.

Enough has been said to characterize briefly the various developments in symbolic logic which stem from the employment of the matrix procedure. We must now attempt an evaluation of these "alternative logics."

(1) Employment of the matrix procedure entails a purely extensional point of view; for all basic relations are defined in terms of truth-value patterns and not in terms of meanings. Implication, in particular, as defined in these calculi, is always "material" implication. Not one of the "alternative logics" provides for a necessary or compelling connection between antecedent and consequent. The interrelation of meanings which furnishes the basis for logical entailment finds no recognition in the matrix procedure.

There is, then, a very clear distinction between an *intensional logic* on one side, and various *extensional calculi* on the other; and in so far as thought, in its cognitive employment, is concerned with meanings and their interrelations, the extensional calculi are not adequate alternatives to intensional logic.

(2) The matrix procedure itself presupposes and rests upon the employment of an intensional logic. It presupposes, for example, the three basic "laws of thought," and these laws—so we have seen in earlier discussions—are rooted in meanings and in the interrelations of meanings. The "law of identity," as employed in the matrix procedure, asserts that "A is identical with A, regardless of the position of A in any given matrix"; i.e., 'A' retains an *identity of meaning* throughout the argument or throughout the construction. The "law of identity" is here substantially identical with the "reiterative law" mentioned in an earlier section. And while this law can be formally stated, its significance lies in its reference to meaning.

An identity may, of course, also be asserted of A and B—as when we stipulate that under certain conditions a complex expression, such as $(\sim p \vee q)$, may be substituted for a simple expression, such as p, without affecting the truth-value of a given statement. What we assert here is not the literal identity of $(\sim p \vee q)$ and p, but their equivalence in terms of truth-functions. We have reference, in other words, to the fact that both symbols designate propositions; i.e., we have reference to the fact that they are not without all meaning; for it is only be-

cause both symbols *designate* or *mean* propositions that we can assert their identity.

If A is identical with B, B is also identical with A. The asserted identity thus entails a "law of symmetry." But this law also expresses an intensive relation; for A is identical with B, and B with A, only because A and B have the same meaning. It is not as marks on paper (or as sounds) that A and B are identical, but only as symbols having identical meanings.

But if A is identical with B, and if, in this same sense, B is identical with C, then—such is the meaning of 'identity'—A is identical with C. The asserted identity thus involves also a "law of transitiveness." And this law, too, is significant only in so far as it pertains to identities of meanings.

Lastly, no calculus can be constructed without recourse to *some* principle of inference; and the only reliable principle of inference, i.e., the one principle which permits logical deductions and compels acceptance of all entailed conclusions, pertains to the interrelation of meanings. It is the principle of the entailment and applicative relations of an intensional logic.

We find thus not only that there is a clean-cut distinction between an intensional logic on one side, and various extensional calculi on the other; but also that the construction of the calculi (no matter how it may be achieved) presupposes the principles and laws of that intensional logic. The various calculi, in other words, far from being adequate alternatives to intensional logic, depend for their very existence upon the prior validity of that logic and upon the interrelation of meanings. Rational thinking, in the last analysis, is the consistent use of concepts and the explication or application, as the case may be, of the meanings of these concepts. The construction and employment of calculi can never replace this basic function of thought, for both are but manifestations of that function itself (cf. Farber).

(3) No calculus "means" anything until it is semantically interpreted; and if a calculus consists of nothing but tautologies, it can never be verified empirically.

Let us assume for the sake of the argument that "material" implication is acceptable as a definition of implication; and let us grant, furthermore, that the "laws of the calculus" are tautologies in Wittgenstein's sense of the term. The calculus can then not be verified empirically, nor does it lead to the empirical verification of anything

As a consequence, it does not really contribute to our knowledge, nor does it provide an adequate basis for truth.

If the calculus as a whole, or any part of it, is to be employed in the search after truth, it must be "interpreted," i.e., it must be *applied* to some realm of experience. The calculus, in other words, becomes an instrument in the service of cognition only if it is augmented through the acceptance of an "applicative principle." This principle, however, is not, and cannot be, a tautology, nor is it definable by means of a truth-value matrix; for its very function pertains to the referential meanings of terms and propositions, and it pertains to these meanings only in so far as the principle itself has meaning.

We discover thus once more that all calculi are of secondary importance only and that they cannot replace a basic intensional logic of which the applicative principle is one of the indispensable laws.

(4) Starting with Wittgenstein's contention that, *as a matter of convention,* any given proposition is either "true" or "false," the advocates of multi-valued "logics" argue, first, that *all* "conventions" are replaceable, and, second, that therefore the truth-values of a proposition are not necessarily restricted to two. They maintain that we can stipulate, *as a convention,* that a proposition has three, four, five, or *n* values, and that, on the basis of such stipulations, we can proceed to construct corresponding "logics." But the question arises, Are the truth-values actually "conventions" which can be modified arbitrarily or is our choice between truth and falsity restricted by something more than a "convention"?

From the point of view of meaning, only two genuine truth-values can be admitted—true and non-true (or false); for they alone are grounded in the compatibility or incompatibility of meanings. All other so-called "truth-values" pertain to something which, strictly speaking, is not the plain bifurcation of truth and non-truth. To call these other values "*truth*-values" is to usurp a name, because of its suggestiveness, without accepting its conceptual signification. And such procedure can lead only to ambiguities and confusion.

Actually, the various calculi "operate" just as effectively without as with reference to "*truth*-values," because it is immaterial for the construction of the matrices what 'T,' 'F,' and other "symbols" employed in a like manner "stand for" or signify—*if* they "stand for" or signify anything. Once this is realized, the calculi will be seen as what they really are: arbitrary devices for the manipulation of semantically meaningless marks on paper. Some of them may be "useful" *if they*

are properly interpreted, i.e., if their symbols are given appropriate semantic meanings; but all such interpretations presuppose the rational nature of thought and the general principles which guide and direct that thought. To speak indiscriminately of "alternative logics" and to regard each calculus in itself as *a* logic is to take the name of logic in vain; for very few calculi—and these only when restrictively interpreted—provide symbolic formulations of the laws and principles of cognitive thought.

The Law of the Excluded Middle

Reference has just been made to the fact that, viewed from the point of view of meanings, two and only two genuine truth-values—true and non-true or false—can be admitted. The problem here involved is, however, of such significance for the development of logic that a cursory reference to it is unsatisfactory and inadequate. A somewhat fuller discussion is called for.

In its most pointed form, the problem in question concerns the validity or non-validity of the "law of the excluded middle," and thus touches upon the very foundation of logic itself. Are *all* propositions *necessarily* either true or false, or is there some third alternative which allows a "truth-value" not included in this bifurcation?

It has been argued that the development of multi-valued "logics" is in itself proof that the law of the excluded middle is neither sacrosanct nor indispensable; that, as a matter of fact, Lukasiewicz, Tarski, and others who have developed "logics" of many "truth-values" have liberated philosophy from the "most obstinate of all superhuman Absolutes," i.e., from the restriction to two truth-values. Just as the development of non-Euclidean geometries put an end to the absolutism of the Euclidean type, so, according to proponents of this argument, the development of multi-valued "logics" has finally brought to an end the "tyranny" of an Aristotelian logic.

The force of this analogy is more apparent than real; for Euclidean geometry is, after all, but one deductive system among others; logic, on the other hand, concerns the nature of deduction itself. The cases are therefore by no means equivalent. And to reason that the development of multi-valued "logics" *proves* that the law of the excluded middle is not indispensable, is to fall into twofold error: (1) The very construction of the proposed alternative "logics" presupposes the validity of the law of the excluded middle; for even so simple a matter as the identification of a symbol within the multi-valued calculus

as this particular symbol and no other is manifestly an instance of the employment of the law in question. (2) The assumption is made that the various calculi actually are alternative *logics*—an assumption which derives what plausibility it possesses from a loose use of the word 'logic,' and which does not survive critical examination.

"Tautologies," in Wittgenstein's sense, can be defined by matrix procedure regardless of what the "values" which constitute the "arguments" stand for. *Any* set of predicates of some specified number will meet the requirements of the matrix scheme so long as the stipulated predicates exhaust all possibilities of admissible values for each "entity" of the calculus. "Calculi" which are constructed on this basis are abstract systems of marks on paper and become significant systems of reasoning only when they are properly interpreted; and a "calculus" is a *logic* only if its abstract symbols can be given specifically logical meanings. Whether or not a given calculus is a *logic* depends, therefore, not on the calculus as such but on whether or not it is an adequate presentation in symbolic form of the entities, predicates, and relations which are characteristic of an antecedently known logic. When this test is applied to the calculus developed in *Principia Mathematica*, it will be seen that any calculus which defines implication as "*material* implication" is not an adequate symbolization of logic, and that the construction of such a calculus presupposes the validity of the very logic for which it is only an inadequate symbolic formulation. Comparable examinations of calculi involving "*strict* implication" lead to corresponding conclusions with respect to these calculi. In general, therefore, the construction of *all* calculi presupposes the validity of an all-embracing logic which is not adequately represented by any of the calculi as such. These calculi, therefore, cannot be genuine alternatives to that one basic logic.

If for some reason the law of the excluded middle should have to be abandoned in logic, then the multi-valued calculi would provide ready-made schemes by means of which any new assignment of truth-values might be handled effectively. But whether or not the law in question must be abandoned in logic cannot be determined by the development of calculi which, in so far as they are at all relevant to logic, are but symbolic formulations or formalized statements of what is valid in logic. If, therefore, the law of the excluded middle is to be abandoned, reasons for this step must stem from considerations other than those of the development of multi-valued calculi. The question is, Are there such reasons?

Let us first clear up a certain ambiguity connected with a presumed denial of the law of the excluded middle. On the one hand, this denial may actually be only the assertion of a subjective impossibility of knowing whether a given proposition is true or false. On the other hand, it may be the assertion of an objective indeterminacy of the proposition itself with respect to truth and falsity. And frequently the latter is asserted on the basis of the former.

In so far as the "denial" of the law of the excluded middle is *meant* only as an assertion of a "subjective impossibility of knowing," it is not a genuine denial of the law and need not concern us here. But if the "subjective impossibility of knowing" is taken as evidence of an "objective indeterminacy," the situation is different. We are then compelled to examine the argument.

Consider, for example, a proposition such as this: "The square root of 2, written as a decimal fraction, contains at least one period of 123456789." We do not know that this proposition is true, nor do we know that it is false. We know, however, that there is no way of determining whether it is true or false, short of a completion of the infinite series of the decimal sequence; and we know, furthermore, that an infinite series cannot be completed. In other words, we are here confronted with a "subjective impossibility of knowing" the truth or falsity of the proposition in question.

Brouwer and other "finitists" conclude from this fact that, actually, the proposition itself is *neither* true *nor* false. But, as here given, the "inference" advances from a "subjective impossibility of knowing" to an "objective indeterminacy," and the argument is a *non sequitur*.

The "finitists" obscure this fallacy by augmenting their argument in a specific way. They insist upon an operational theory of meaning and demand that the defining operation be at least theoretically performable. Nothing is to be admitted into a system of thought that cannot be defined by the operations which generate the system, or that cannot be "constructed" through a finite number of operational steps. The generic "law of +1," for example, determines the character of all members of the class of positive integers so that, on the basis of this law, we can say of any given number whether it is a positive integer or not. But should actual "construction" or definition through finite operations be impossible, then, according to the "finitists," we cannot decide whether or not a given "entity" possesses the "predicate" ascribed to it; i.e., we cannot decide whether a proposition asserting that the "entity" possesses the "predicate" is true or false. And this,

according to the "finitists," means that the proposition ascribing the "predicate" to the "entity" is neither true nor false.

But this argument is also unacceptable. If its presuppositions—which are: the operational theory of meaning and the performability of the defining operation—are strictly adhered to, then the propositions called "neither true nor false" are in reality meaningless; for their meaning is not definable within the operational system relative to which they are adjudged to be neither true nor false. The argument, in other words, has no bearing upon the validity of the law of the excluded middle; for that law, in so far as it pertains to the bifurcation "true-false," is concerned with propositions; and that which is meaningless cannot be a proposition.

But let us approach the matter from a still different angle. It may be argued that propositions about the future—such as, "Henry will graduate next June," "The Republicans will carry Nebraska in the next presidential election"—are as yet undetermined in so far as their actual truth-values are concerned; that, the world being what it is, the contingency of events could actually verify or falsify them; and that, therefore, they are at present neither true nor false. They illustrate, in other words, situations in which the law of the excluded middle does not hold.

But even this argument is not convincing, for in order that the propositions in question be true today, all that is needed is that the events described actually happen at the time indicated. Whether or not there are now facts available which make an unqualified prediction possible is beside the point. The availability or non-availability of such facts determines only whether or not *we know in advance of the events themselves* which propositions are true and which are false. But our advance knowledge is not essential to the truth or falsity of the propositions themselves.

A corresponding argument pertaining to propositions dealing with events of the past is equally ineffective as disproof of the law of the excluded middle.

The crux of the matter is this: the law of the excluded middle asserts that any given "entity" S either has the "predicate" P or does not have the "predicate" P; "S is either P or not P." The denial of the law is equivalent to the assertion of a factual indeterminacy of S of such a type that it cannot be said of S that it has P or that it does not have P. If this is correct, then the answer to all arguments against the law of the excluded middle is simple. To say that S *has* P is to

affirm that it actually possesses a specific determinate quality; but to say that it does *not* have P is to assert, not the presence of some other specific quality called "not P," but merely the *absence* of the quality P. The "not" is here not part of the definition of a quality, "non-P," but only the negation of the affirmed presence of P. It follows that if any S is so indeterminate in nature that it cannot possess a specific quality P, it is at once describable as "not P"; i.e., such an S can be characterized by the denial of the presence of P. The law of the excluded middle remains in full force.

If this is clearly understood, then additional light is cast upon the problem of the truth-values of propositions which deal with future events, i.e., of propositions having the form of "S will be P." Propositions of this type are true only if, at the specified time, S actually is P. Under all other conditions—and these include the factual indeterminacy of S—S is not P, and the proposition "S will be P" is false. There is no escape from the strict bifurcation of the law of the excluded middle.

The argument here developed is independent of any particular theory of truth we happen to accept. Whatever our criterion of truth may be, the assertion that a given proposition is true merely means that it conforms to the truth-conditions which we have stipulated; and the assertion that the proposition is false means that it does not conform to these same conditions. Every other consideration is irrelevant.

Formalization of Logic

Throughout the present chapter we have encountered the contrast between intensional logic and various formalized calculi; that is to say, we have encountered in a new form the old differentiation between interests in logic which center primarily around meanings and the interrelations of meanings, and interests which center primarily around form and formal relations. We have seen that formalized calculi which are developed without concern for meaning are not adequate as formulations of logic, that they become *logic* only if they can be appropriately interpreted to yield an entailment of meanings; and we have seen, finally, that no calculus of the customary type meets all the requirements for such an interpretation. Does this mean that logic cannot be formalized? Or does it mean merely that efforts at formalizing it must necessarily fall short of the goal so long as they disregard the interrelations of meanings? The answer cannot be in doubt. Significant form, after all, has no independent existence. It is

always intertwined with meaning and is, in fact, but the formal aspect of the interrelations of meanings viewed abstractly. In modern terminology, syntax (logic) and semantics (meaning) belong indisputably together. Hence, if logic is to be formalized, its formalization must include also the formalized aspects of meaning; and a complete formalization of meaning—if it can be achieved—must culminate in an adequately formalized logic.

If I understand Carnap correctly, he is now engaged in the development of just such an all-comprehensive formalized system as is here suggested. His aim is to construct a "calculus" in such a way that the principal signs can be interpreted only as *logical* signs, i.e., as indicating normal logical relations, and that all non-normal interpretations are precluded.

Carnap realizes that this goal cannot be reached unless new basic concepts of semantics and syntax are introduced into the formalized scheme of symbols. The customary calculi, taken by themselves, are insufficient. The propositional calculus of *Principia Mathematica*, for example, which is the same as that developed by means of the matrices of normal truth-value tables (Wittgenstein), admits two kinds of non-normal interpretations, one in which every statement in the calculus is true in semantics, and one in which at least one statement in the calculus is false in the realm of meanings.

"Normal interpretation," as here understood, is possible only when each and every sign for a specific connective in the calculus is also a sign for the same specific connective in the realm of meaning. If, for example, a sign of negation in the calculus should violate one of the rules defining negation in the field of semantics, then that sign would not be a sign of negation in semantics, and the calculus involving it would have no normal interpretation in terms of semantics.

The customary way of constructing a calculus is to lay down rules which define and govern implication, and then to "derive" only those properties and relations of propositions which are determined by these rules. A calculus, therefore, can be an adequate formalization of logic only to the extent to which the stipulated rules define logical relations.

Now, as Carnap has shown, the logically significant conditional ("if . . . then") relation between two propositions, if considered with respect to the truth-values of these propositions themselves, defines four and only four elementary relations; and it, in turn, is fully defined only by all four of these relations taken together. In a schematic form: (1) If p is true, then q is true; i.e., it cannot be both that p is true

and q is false; i.e., p semantically *implies* q. (2) If p is true, then q is false; i.e., it cannot be both that p is true and q is true; i.e., p semantically *excludes* q. (3) If p is false, then q is true; i.e., it cannot be both that p is false and q is false; i.e., p is semantically *disjunct with* q. (4) If p is false, then q is false; i.e., it cannot be both that p is false and q is true; i.e., p is a semantic *implicate* of q. However, in calculi of the customary kind only the relations of "implies" and "is an implicate of" are definable by the rules governing implication, the latter relation being the inverse of the former. The semantic relations of "exclusion" and of "being disjunct with" are not definable in terms of the rules governing implication and can therefore not be formalized in the calculi in question.

Since the set of rules which provides the basis for the customary calculus is in itself complete, the deficiencies just referred to cannot be remedied by adding rules to the original set. A new approach to the formalization of logic must therefore be attempted; and this approach Professor Carnap provides for both a propositional and a functional logic.

Through the introduction of a "disjunctive rule of inference" and of a "disjunctive rule of refutation" he augments the customary propositional calculus so as to preclude the possibility of non-normal interpretations of the connectives. The propositional *calculus* is thus restricted to the range of propositional *logic*. In a similar manner and through the introduction of corresponding rules, Carnap accomplishes a comparable restriction for the interpretation of the functional calculus. In both cases, the *logic* is presupposed and the *calculus* is then constructed so that it permits true and normal interpretation only in terms of the respective logics.

The details of Carnap's procedure are too technical to concern us here. For our purpose it is sufficient to recognize that, in principle, the interrelation of meaning (semantics) and logic (syntax) has been acknowledged, and that it is the meaning-side of logic (formalized though it may be in Carnap's system) which provides the basis for an adequate formalization of logical relations. To put it differently, there is nothing in the proposed formalization of logic that contradicts the thesis underlying the present chapter—the thesis, namely, that logic can be truly understood only in the context of meaning, that it is the syntax of meanings.

CHAPTER VI

MATHEMATICS

Mathematics, like logic, is rooted in the realm of meanings and arises out of the sensory-intuitive stratum of first-person experience. Its key concept is *number*, and its basic relations are *relations of cardinal numbers*. But what are "numbers"? And what, in particular, are "cardinal numbers"? The Frege-Russell attempt to derive mathematics from logic has come to nought because, as Hilbert could show, the subject matter of mathematics is specifically different from the subject matter of logic, for "number" is not a concept of logic. What, then, is the basis of mathematics?

The Experiential Basis of Numbers

The rudiments of mathematics appear first in close connection with language and language forms. In fact, linguistic devices denoting plurality are not always clearly distinguishable from "number" words proper.

It is possible, as Cassirer has suggested, that the experiential situation of "speaker" and "person spoken to" provides the ultimate basis for number concepts, for many primitive languages show that the act of separation, as it develops in connection with the opposition "I-thou," advances readily from "one" to "two"; that a further significant step toward a number system is taken when the "person spoken of" is added as "three"; but that beyond this the power of quantitative discernment seems paralyzed in some primitive tribes. They literally do not count beyond "three."

Be that as it may, number systems in the full sense of the term are developed only in conformity with the needs and desires of primitive man and in closest contact with "countable" objects. Concrete situations rather than abstract notions constitute the basis upon which the superstructure of numbers is erected.

In the languages of primitive man two aspects of number—a "distributive" reference and a "collective" meaning—are clearly distinguishable and, corresponding to these two aspects, we encounter on a higher level of number concepts the idea of "ordinals" and the idea

of "cardinals"; and without "ordinals" and "cardinals" arithmetic, as we know it, is impossible.

Analysis shows that "ordinals" and "cardinals" are so intertwined that they can never be completely separated, for where there is a "first," there is necessarily a "one": the "one first"; where there is a "second," there is also a "two": the "first" *and* the "second"; where there is a "third," there is also a "three": the "first," the "second," and the "third," and so on for all other numbers. Conversely, where there is a "one," this "one" is also a "first"; where there are "two," there is necessarily a "first" and a "second"; where there are "three," there is a "first," a "second," and a "third," and so on for all numbers. The one meaning always involves the other.

Upon closer inspection we find, however, that the cardinal meaning presupposes the ordinal meaning in a way in which the latter does not presuppose the former; for the cardinal meaning cannot be defined in the strict quantitative sense without recourse to the ordinal meaning, while the ordinal meaning, although always implying the cardinal meaning, can be defined without reference to it. The ordinal meaning, therefore, is more fundamental than the cardinal meaning and is logically prior to it. But once the cardinal number has been defined, the notion of order and succession, as expressed in the ordinal meaning of numbers, becomes relatively insignificant in the field of mathematics and need concern us no longer until the notion of "transfinite" numbers changes the situation.

Experientially, the meaning of numbers, like all other meanings, is grounded in the variegated manifoldness of first-person experience. When "I" open "my" eyes, "my" visual field is broken up into distinct forms of many hues and shades; and while "I" am awake, sounds and odors, feelings and thoughts and memories crowd in upon "me." "I" am aware of, and distinguish, various objects within the range of "my" experience. "I" can, and do, identify "this" object as *this*, and "that" object as *that*; and each object so identified is a self-identical "something" for "me"; and, in this sense, it is a "one." But, surely, as "I" identify an object as a *this*, "I" distinguish it from everything which is *not this*; i.e., "I" distinguish it from a *that* which is *other than the this*. This distinction, however, implies that "I" have transcended the "one" by recognizing or acknowledging "the other." In the very act of name-giving "I" encounter thus a unitary situation in which "I" discern the "one" *and* the "other," in which "I" discern, in other words, a "one" *in relation to* "another one." And this rela-

tion, which transcends the self-identity of the "one" and provides the experiential basis for a "two," makes numbers and number systems possible.

Let us look at the matter from a different angle.

The distinction of different "ones" within the manifoldness of experience does not in itself imply the specific numerical differences of "two," "three," "four," "five," etc. The "many" which we discern in experience are, as *many*, only an aggregate of something *and* something *and* something *and* . . .; i.e., they are an aggregate of "one" *and* "one" *and* "one" *and* "one" *and* . . ., where the word 'and' is an enumerative conjunction and does not have the meaning of the mathematical symbol + (plus). The notion of "many," therefore, remains indeterminate. It lacks quantitative preciseness and does not specify *how many*.

However, the objects of experience which, in the aggregate, are the "many," can be arranged in a certain order so that there is a *first* object, a *second* object, a *third* object, and so on. That is to say, objects of experience can be "enumerated" or "counted." Through a synthetic and irreducible act of interpretation we thus create the ordinal meaning of numbers and prepare the way for the development of mathematics. The creative act which transforms an indeterminate "many" into an ordered sequence of a "first," a "second," a "third," and so on, gives meaning and significance to whatever the mathematician may do, for it creates the basis of our number system: the sequence of *natural numbers* or of *positive integers*.

Furthermore, through this generating act of counting, the first mathematical operation, that of addition, also comes into being; for 1 (or +1) means: beginning with "zero," count "one"; and 1+1 means: beginning with "zero," count "one" and then, beginning with the first "one," count another "one." On the same basis, 1+1=2 means that to count "one," starting from "zero," and then to count another "one," starting from the first "one," is *the same as* counting "two," starting from "zero"; while 2+3=5 means that to count "two," beginning from "zero," and then to count "three," beginning from the "two," is *the same as* counting "five," beginning from "zero."

So far we have identified counting as that particular synthetic act of mind which creates the basis of all mathematics, the sequence of natural numbers, but we have not yet defined 'number,' nor have we given an adequate interpretation of its "cardinal" meaning. The mathematician, therefore, cannot rest satisfied with what we have

done. Mathematics, after all, is a system of strictly logical relations and rigorously logical dependencies. "Acts of mind," no matter how significant or creative they may be, can have no place in such a system; for that which generates or creates a logically rigorous system cannot itself be an integral part of that system. We must advance, therefore, from the act which generates numbers to the numbers themselves and to their essential character and their basic interrelations.

Cardinal Numbers

The key concept of mathematics, so we have said, is the "cardinal number." But what is a "cardinal number"?

Let us return for a moment to a consideration of our first-person experience of shapes and forms and "countable" objects. We have seen that these objects of experience can be arranged in orderly sequences of a "first," a "second," a "third," a "fourth," and so on. We must note now that they can also be *grouped together* in "classes"; that we can "think them together" in different ways or speak of them "collectively" as such and such a "class." The range of the "class" depends in each case on some particular interest or purpose we have in mind. We can thus group together or classify flowers as "red flowers," "blue flowers," "yellow flowers," and "white flowers"; or as "wild flowers" and "garden flowers"; as "flowers of the tropics," "flowers of the prairies," "flowers of Brazil," "flowers of Nebraska," etc. The formation of such "classes" depends ultimately as much upon a synthetic and irreducible act of mind as does the arrangement of objects in ordinal sequences. And in so far as this new act of mind provides the basis for the cardinal meaning of numbers it is as fundamental as is the act of counting.

The act of creating collective wholes or "classes" is, however, insufficient. If a system of mathematics is to emerge, we must also "compare" the classes by "pairing" their elements, i.e., by putting every element of one class into a one-to-one relation with a corresponding element of another class. If all elements can actuallly be "paired," the two classes are "equivalent"; but if elements of one class are "left over" and cannot be "paired" with elements of the other class, then the class containing the "unpaired" elements consists of "more" elements than does the class with which it was compared. The process of "pairing" thus defines "degrees of quantity"; and upon this basis the cardinal number can be defined.

Suppose now that we have formed the class "all horses on this ranch." We can then put a saddle on every horse and thus form the class "saddles on the horses on this ranch." This class of "saddles" is obviously equivalent to the class of "horses on this ranch"; for every element of the one class stands in a one-to-one relation to some specific element of the other class, and no element is "left over." Next we may place a rider in every saddle, thereby forming another class which is equivalent to the class of "horses on this ranch." Still other equivalent classes may readily be formed by placing on the head of each rider a cowboy hat, and then putting a feather in each hat.

No matter how much the elements of these various classes differ in quality, the classes themselves, as *equivalent* classes, have something in common—something which is "invariant" and which constitutes their "equivalence." This "something" is the specifically determined "manyness" or "number" of their elements. A "number," therefore, is any specific "class of equivalent classes" or, more concretely, any *specific* limitation of the "many." Conversely, every possible way of limiting the "many" (or of defining a new class of equivalent classes) yields a new number (cf. Russell).

The definition of 'number' just given involves two aspects, for we have spoken of a "specific limitation of the many" and also of a "class of equivalent classes"; but whichever aspect we emphasize, it leads back to the idea of ordinal numbers as an indispensable presupposition of cardinal numbers. If we stress the idea that each cardinal number is a specific limitation of the "many," then it is evident that the recognition of such a limitation—in particular when one "limitation" is to be distinguished from another—presupposes a "counting" of the elements involved and is therefore impossible without prior knowledge of ordinal numbers. If we emphasize the notion that cardinal numbers are "classes of equivalent classes," we must assume that the equivalent classes themselves are given as a plurality; that is to say, we must assume that there exists a "first" class which is equivalent to a "second" class; that both are equivalent to a "third"; that all three are equivalent to a "fourth," and so on. The idea of ordinal numbers thus appears once more as the essential minimum without which an analysis of "numbers" is impossible. Add to all this the obvious fact that no matter how many "equivalent" classes we discover, the "pairing" of elements by itself never results in such purely numerical concepts as "three," "eight," "ninety-seven." In other words, the question, "How many?" i.e., the basic question per-

taining to cardinal numbers, can never be answered satisfactorily by replying, "Just as many."

Every attempt to overcome this difficulty by purely logical means must come to naught because numerical specificality is not a concept of logic.

Let us assume, for the sake of the argument, that a class m of equivalent classes is given; and let us assume also that we are able to form a class n of equivalent classes such that every class of n contains "one" more element than does every class of m. 'More' is defined by the process of "pairing" referred to above. Let us assume, finally, that we can form also a class $(n+1)$ of equivalent classes so that every class of $(n+1)$ is by "one" greater than every class included in n. Since m is arbitrarily chosen, it may be *any one* of all possible "classes of equivalent classes," i.e., it may be any one of all possible cardinal numbers. The relationship which we have stated for n and for $(n+1)$ with respect to m must therefore hold with respect to all cardinal numbers. If this is so, then it is possible to arrange all classes of equivalent classes in a progressive series of such a type that every class of equivalent classes is by "one" greater than its immediate predecessor. And within this series every class of equivalent classes, i.e., every cardinal number, has its specific numerical value.

However, this numerical value of the cardinals can be derived from the serial arrangement only if we take into consideration the ordinal meaning of numbers. That is to say, 2, 4, 8, 20, 100, etc., as classes of equivalent classes, possess definite "numerical" values only because they occupy, respectively, the second, the fourth, the eighth, the twentieth, the one-hundredth, etc., place in the established sequence. And thus, despite all logical efforts to prove the contrary, it is the ordinal number which determines the specific values of the cardinals, and "counting" turns up once more as the ultimate basis of all number concepts. And through the process of "counting" the whole structure of mathematics remains solidly grounded in the contextual meanings of first-person experience.

Mathematical Induction

Once we accept the sequence of cardinal numbers as defined in the preceding section, it is obvious that the generic law "$+1$" determines adequately and completely all elements of the series. Starting from "zero," this law "generates" every whole integer in the sequence 1, 2, 3, 4, 5, ... Moreover, the series thus generated is unending; for

no matter what integer n has been reached, we can always write the next one as $n+1$. The series of integers is thus *infinite*. And this infinite series forms the basis for one of the most fundamental principles of mathematics—the principle of *mathematical induction*.

Mathematical induction must be clearly distinguished from "empirical induction." The latter proceeds from a limited number of observed phenomena of a certain type to a general statement concerning *all* phenomena of this type. The "inductive leap" from *some* to *all* finds no justification in the principles and laws of logic and never yields absolute certainty. A single additional observation may contradict the "generalization" and may nullify it as a "universal truth." The principle of mathematical induction, by contrast, precludes the possibility of an exception and thus provides *absolute certainty* for any generalization based upon it.

Briefly stated, the principle of mathematical induction asserts that whenever a theorem can be proved for a whole number n and for $(n+1)$, it is true for every whole number $\geqq n$. That is to say, whenever a theorem is verified for some specific class from which all other classes of a series can be built up by the use of accepted principles of construction, and if it can be shown that the progression from that class does not contradict the theorem, then the theorem is true for any class that may be derived from the first. Let me illustrate what is meant.

Consider the following theorem of Euclidean plane geometry: "The sum of the angles in a polygon of $n+2$ sides is n times 180 degrees." The theorem is universal in scope; but how can we be sure of this? Were we to rely upon "empirical induction," we would examine a fairly large number of polygons selected at random (say, a hundred, or a thousand, or perhaps ten thousand), and we would try to ascertain whether or not all of the "observed" cases conform to the stipulations of the theorem. If we found that they did, we would "infer" that the theorem is *probably* true in *all* cases. However, in view of the infinity of possible cases, this generalizing "inference" would never yield absolute certainty. We would know with certainty only that all polygons so far examined possess the characteristics stipulated in the theorem; but the very next one which we examine *might* reveal different relations. The compelling certainty of mathematical inference would be lost in the midst of such uncertainties.

If we have recourse to the principle of mathematical induction, the situation is quite different; but the employment of this principle pre-

supposes: (1) that we discard all *random* examinations of polygons; (2) that we arrange the polygons in a series in conformity with some principle of construction—such as adding always one, and only one, side to get the next number of the series; (3) that we prove the theorem for the first member of the series; and (4) that we prove it for the member following the first, thus showing that the law of construction does not contradict the theorem.

In our example, if $n=1$, the polygon is obviously a triangle. We can now arrange all polygons in a series so that each additional polygon has one, and only one, side more than its immediate predecessor. The second polygon in our series is thus a quadrilateral, and the third a pentagon, and so on. We can then show that the theorem is true for $n=1$, i.e., that it is true for the triangle. We know from Euclidean geometry that the sum of the angles in a triangle is indeed 180 degrees. For the quadrilateral, $n=2$. According to the theorem, the sum of the angles in the quadrilateral should be 2 times 180 or 360 degrees. If we draw the diagonal, we observe that the quadrilateral consists of two triangles—which means that the theorem is true also for the case of $n=2$. The principle of construction does not contradict it. We now infer that the theorem must be true for *all* polygons.

We can put this inference to the test for polygons of any number of sides and it will be found to be true, for all polygons can be derived from the triangle by adding always one more side; i.e., by always increasing n by 1. For the case of $n=3$, or for the pentagon, for example, we can show that this figure can be broken down into a quadrilateral and a triangle. Since the former has the angle sum $2 \times 180°$ (as already proved), and since the latter has the angle sum $1 \times 180°$ (as shown from elementary geometry), we obtain 3×180 degrees as the angle sum for the pentagon—which is what the theorem predicted.

Consider a second example. The theorem of "arithmetical progression" asserts that for every value of n, the sum $1+2+3+\ldots+n$ of the first n integers is equal to $\frac{n(n+1)}{2}$; in brief:

$$1+2+3+\ldots+n=\frac{n(n+1)}{2}.$$

Let us see first what this theorem asserts. On the left-hand side of the equation we have the sum of the first n integers. The first integer is $+1$; the second is $1+1$; the third is $1+1+1$; the nth is $(n-1)+1$.

"+1," in other words, is the law of construction of this series. The theorem now asserts that the sum of the first n integers is exactly $\frac{n(n+1)}{2}$, no matter what natural number n is. The theorem, in other words, makes an assertion which is infinite in scope. No "empirical induction" can ever justify it. To be more specific, the theorem asserts

$$\text{for } n=1, \qquad 1=\frac{1(1+1)}{2}$$

$$\text{for } n=2, \qquad 1+2=\frac{2(2+1)}{2}$$

$$\text{for } n=3, \qquad 1+2+3=\frac{3(3+1)}{2}$$

$$\text{for } n=4, \qquad 1+2+3+4=\frac{4(4+1)}{2}$$

and so on.

As may be seen from these equations, the theorem is true for $n=1$, and also for $n=2$; it is not contradicted by the principle of construction which generates the series of sums. It is therefore true for *all* cases of n.

The certainty of this "inductive" inference presupposes, of course, the validity of the principle of mathematical induction; but what guarantees the validity of this principle? The principle is admittedly not derivable from the laws of an intensive logic. Poincaré regarded it as a "synthetic judgment a priori" belonging exclusively to the field of mathematics. Peano introduced it as Postulate V of his abstract mathematical science. Voss tried to show that it is implied in certain general notions of ordered sequences. And, indeed, the answer to our question lies in the general field of ordered sequences.

In any "simply ordered sequence" (such as the sequence of integers) a specific "law of construction" (such as "+1") determines the elements of the sequence in such a way that each given element has an immediate successor and that therefore any desired element n of the sequence may be reached by a finite number of applications of the "law of construction." If in such a sequence it can be shown that an assertion A_r is true for any element n, and also that the "law of construction" makes the assertion true for the immediate successor of n, then there can be nothing in the sequence itself which contradicts

(or falsifies) the assertion; for there can be nothing in a sequence that is not determined by the "law of construction" which generates the sequence. What makes the principle of mathematical induction valid is, therefore, the fact that in mathematical sequences nothing exists which is not created as an element of the sequence by the very "law of construction" which gives rise to the sequence itself.

The Ideal of a Closed Number System

We shall assume as given the sequence of positive integers 1, 2, 3, 4, ... Within this sequence the simple operations of *addition* and *multiplication* are possible. They can be carried out, however, only so long as certain fundamental laws—the laws of arithmetic—are observed.

In order to state these laws in their full generality, we use letters (a, b, c, . . .) representing *any* integer rather than symbols (1, 2, 3, etc.) designating *specific* integers; and we write:

(1) $a+b=b+a$ Commutative law of addition.

(2) $ab=ba$ Commutative law of multiplication.

These two laws state that, in addition or multiplication, the order of elements may be interchanged.

(3) $a+(b+c)=(a+b)+c$ Associative law of addition.

(4) $a(bc)=(ab)c$ Associative law of multiplication.

Law (3) states that the result of an addition is the same whether we add the sum of second and third numbers to the first number, or the third number to the sum of the first and second numbers. Law (4) states corresponding conditions for multiplication.

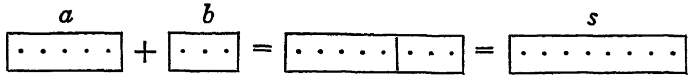
(5) $a(b+c)=ab+ac$ Distributive law.

This last law states that to multiply a sum by an integer is the same as multiplying each term of the sum by the integer and then adding the products.

All five of these laws are obvious and can be readily applied to specific integers; but they are not trivial. They do not necessarily hold true for "entities" other than integers. If, for example, concentrated sulphuric acid is added to water, the result is a dilute solution; but if water is added to concentrated sulphuric acid, the result may be an explosion. The commutative law does not hold for chemical substances.

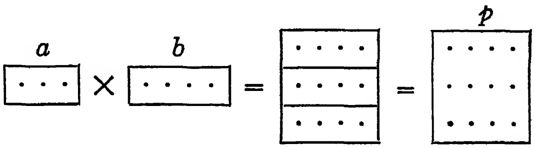
The operations of addition and multiplication are grounded in the sensory-intuitive range of first-person experience and can be understood concretely on the basis of "counting." Addition has thus an

experiential basis in manipulations of the kind illustrated in the following scheme (cf. Courant and Robbins):



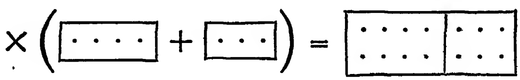
i.e., to add the integers a and b, we place the respective "boxes" of dots end to end and remove the partition between them, thus creating the "box" of integers representing the sum, s, of a and b.

Multiplication has a corresponding basis in manipulations such as these:



i.e., for every dot in "box" a we place a "box" b in a rectangle and remove the partitions, thus creating the product, p, of $a \times b$.

The arithmetic laws (given above: 1-5) can be verified through appropriate manipulations of the dots and "boxes." The distributive law, for example, is illustrated thus:



Once the operation of "addition" is accepted, we can define the relation of *inequality* of two integers. The integers a and b are unequal, if b can be derived from a by adding to a an integer c; i.e., if $b > a$ ("b greater than a"), then $b = a + c$. In other words, c is the integer which is equal to the difference between b and a: $c = b - a$. And this equation defines the operation of *subtraction.*

Since $(a+b)-b=a$, addition and subtraction are *inverse* operations; and since in some cases b may be equal to a, i.e., since in some cases $b=a$, $b-a=a-a$. This operation results in a number which is not represented in the sequence of positive integers 1, 2, 3, 4, ... Hence, if the operation $a-a$ is to be possible at all, we must augment the sequence 1, 2, 3, 4, . . . by introducing the integer "zero"—thus creating the sequence 0, 1, 2, 3, 4, ...

Resorting once more to the sensory-intuitive illustration of dots and "boxes," "zero" is a "box" with no dots in it. Manipulations

involving this *empty* "box" show that $a+0=a$, that $a\times 0=0$, and that the five basic laws of arithmetic remain in full force.

What is important here is the fact that, *in order to make certain mathematical operations possible*, it has become necessary to expand the number system by adding a "zero" to the sequence of positive integers. The philosopher must recognize here the emergence of a mathematical ideal—*the ideal*, namely, *of a closed number system*, i.e., the ideal of a system of numbers so complete and all-comprehensive that no mathematical operations lead to results which fall outside the scope of the system. This ideal of the closed number system has been one of the most fruitful ideas in the field of mathematics and has been both driving force and guiding light in large fields of mathematical invention and construction. It is the basic idea of integration in the field of number theory. But let us be more specific.

We said above that a and b are unequal if b can be derived from a by adding to a an integer c, i.e., a and b are unequal if $b > a$. We now add that a and b are unequal also if a can be derived from b by adding to b an integer c, i.e., if $a > b$. If, as before, we express this relation by writing $c=b-a$, then this equation has no solution within the range of positive integers.

We now face two alternatives: Either we confine the meaning of inequality to the special cases of $b > a$ and rule out the possibility of an inequality of the type $a > b$—an alternative which is not only absurd as far as the meaning of "inequality" is concerned, but which also restricts the operation of subtraction to cases of $b > a$ and therefore excludes it as a true inverse of addition; or we enlarge again the number system—this time through the addition of *negative integers* $(-1, -2, -3, -4, \ldots)$—so as to make subtraction universally possible. Confronted with these alternatives and inspired by the ideal of a closed number system, the mathematician does not hesitate. He creates the *generalized domain of positive and negative integers* in order to preserve the twofold possibility of inequality ($b > a$ and $a > b$), and in order, furthermore, to make possible a solution of the equation $b-a=-(a-b)$.

This extension of the number system requires, however, the introduction of at least one additional rule—the "rule of signs"; $(-1)(-1)=1$. If this rule is not accepted or if some other "rule" is accepted instead, operations with negative numbers lead to violations of the five basic laws of arithmetic. Here we encounter, therefore, a specific restriction of the expanding number system. Whatever

we add to the original sequence of positive integers must be added in such a manner that the five basic laws are not violated; i.e., it must be added in such a way and under such safeguards that the resultant system as a whole remains self-consistent. To the ideal of a closed number system is thus added the ideal of a rigidly self-consistent system based upon the five basic laws.

The "rule of signs" cannot be derived from any other rules or laws either of logic or of mathematics. It can be justified only in the sense that without it the commutative, associative, and distributive laws cannot be preserved in a generalized number system. It is the expression of a synthetic act of mind, of a resolution.

When we discussed the categories of order in first-person experience (Chapter III), we pointed out that, experientially, the category 'quantity' has two aspects: (a) the *enumerative* aspect of "so many," (b) the *intensive* aspect of "so much." And we pointed out, furthermore, that both of these aspects together constitute the experiential basis of mathematics.

Now, the enumerative aspect of quantity finds recognition in counting and in the operations based upon counting: addition, multiplication, and subtraction. The intensive aspect of quantity, which we have not yet discussed, provides the basis for "measuring," and for the operation of *division.*

In order to *measure* any given quantity, we must first select a *unit* of measurement as our "standard." This means that we actually change the "measuring" into a "counting"; for we measure by counting the number of "units" of our standard which are required to "exhaust" the quantity. If the given quantity is "length," and if our "unit" is the "foot," then we measure the quantity by counting the number of "feet" it takes to cover the whole "length."

Experience soon reveals that counting the "units" does not always lead to satisfactory results. A given quantity is not always measurable in exact "units." A given length, for example, may not be exactly 4 feet, nor exactly 5 feet, but somewhere between 4 and 5 feet. In order to make our measurements more accurate, we must introduce "sub-units," i.e., we must *divide* the original unit into smaller "units"—the "foot" into "inches," the "meter" into "centimeters," the "hour" into "minutes," the "pound" into "ounces," and so on. Whatever our "units" or "sub-units" may be, for the sake of more accurate measurements we *divide* "unit" into a number n of equal parts. This division we designate symbolically by $1/n$.

If now a given length "measures" 2 feet 4 inches, we can state this as 28/12 feet; and if it "measures" 7 inches, we can state this as 7/12 foot. In general, we can express the "measure" of a quantity by the quotient m/n. This symbol signifies the operation of *division*—an operation, incidentally, which is the inverse of multiplication.

If m and n are positive integers, m/n is called a *rational number.* However, the quotient, m/n or, in a specific case, b/a, exists *as an integer* only if a is a factor of b (e.g., $4/2=2$). If a is not a factor of b (e.g., 3/5), we enlarge the domain of positive and negative integers by regarding the *symbol b/a itself as a number.* And we call this new number a *fraction.* This extension of the domain of integers is possible *provided* we subject the new numbers to at least one additional rule—the rule, namely, that $a(b/a)=b$. The fraction b/a is then, by definition, a new number.

Now, if "addition," "multiplication," and "equality" are appropriately defined for the rational numbers, then the extension of the number system (which is required in order to make division possible), i.e., the introduction of *fractions,* does not violate the basic laws of arithmetic and is therefore a permissible and legitimate extension of the domain of positive and negative integers. Thus, if we define 'addition' by $\frac{a}{b}+\frac{c}{d}=\frac{ad+bc}{bd}$, 'multiplication' by $\frac{a}{b}\cdot\frac{c}{d}=\frac{ac}{bd}$, and 'equality' by $\frac{a}{a}=1$ and $\frac{ac}{bc}=\frac{a}{b}$, then the five basic laws can be shown to remain valid in the new domain of rational numbers. The commutative law for addition, for example, now takes the form of the equation $\frac{a}{b}+\frac{c}{d}=\frac{c}{d}+\frac{a}{b}$, and its proof is the sequence of equations

$$\frac{a}{b}+\frac{c}{d}=\frac{ad+bc}{bd}=\frac{cb+da}{db}=\frac{c}{d}+\frac{a}{b}.$$

There is only one restriction imposed upon the free operation of division: division is possible only if $a \neq 0$ or, to put in differently, *division by zero is impossible.*

The domain of rational numbers, that is, the system of integers and fractions, positive and negative, is now complete. It is a closed system in the sense that within it all the so-called *rational operations* (addition, subtraction, multiplication, and division) may be performed without ever leading to results which lie outside the system. All operations, furthermore, are completely governed by the five basic

laws of arithmetic—the commutative laws, the associative laws, and the distributive law. In fact, the expansion of the original sequence of positive integers into the domain of rational numbers was permitted and carried on under the one condition: that those laws remain in full force. The closed system thus constructed is, in mathematical terminology, a *field* (cf. Courant and Robbins).

Further Expansion of the Field

In the preceding section an attempt was made to discuss the development of a closed number system in connection with the practical problems of "counting" and "measuring." This was done because it is evident from the history of mathematics that practical needs have often been the stimulus to abstract systematic endeavors. Now, if we return to the practical side of mathematics, we soon encounter situations which necessitate a still further expansion of the number system; for we encounter problems pertaining to quantity which cannot be solved with the mathematical apparatus so far discussed.

When we measure a quantity—such as a length b—with a standard a, it may happen that b is an exact multiple of "unit" a, i.e., $b=ra$ (where r is an integer); or it may happen that b is not an integral multiple of a itself but of some number m of the n "sub-units" of in which case $b=ma/n=\frac{m}{n}a$. For all cases in which $m=n$ or $m=rn$, $b=\frac{m}{n}a$ reduces to $b=ra$. The case of $b=ra$ is therefore a special case $b=\frac{m}{n}a$.

If for any two values a and b the equation $b=\frac{m}{n}a$ holds, the quantities represented by a and b are said to be commensurable. Their "common measure" is the fraction m/n, which goes n times into a, and m times into b.

Now, as the Pythagoreans well knew, not all quantities are commensurable. The diagonal of a square, for example, is ***incommensurable*** with its side; for if we assume the sides of a square to be "unit," and if we designate the diagonal as x, then, according to the Pythagorean theorem, the equation $x^2=1^2+1^2=2$ should hold, i.e., x should be $\sqrt{2}$; but no rational number equal to $\sqrt{2}$ exists. If there were a rational number exactly equal to $\sqrt{2}$, it would be reducible

to lowest terms and statable as p/q, where p and q are integers having no factors in common except ± 1, and where $q \neq 0$. In other words, $p/q = \sqrt{2}$. From this it would follow that $p^2/q^2 = 2$ and that $p^2 = 2q^2$. Since q is an integer, q^2 must be an integer; and p^2, being twice the integer q^2, must be even. Since p and q have no factor in common, *q must be odd*, for if q were also even, p and q would have a common factor 2. Since p is even, $p = 2x$ (where x is an integer). Substituting this value for p, we get $(2x)^2 = 2q^2$, and therefore $4x^2 = 2q^2$, or $q^2 = 2x^2$. In this equation, q^2 is twice the integer x^2 and must therefore be even. But if q^2 is even, q must also be even. The assumption, therefore, that $\sqrt{2} = p/q$ is a rational number leads to the contradictory conclusion that q is *both* odd *and* even—which means that $\sqrt{2}$ cannot be a rational number. Thus we are forced either to admit that some quantities cannot be measured, i.e., that they cannot be manipulated mathematically, or to enlarge still further the number system in order to provide solutions for the mathematical operations here involved.

There are various ways in which the new numbers may be introduced or defined. In *A Philosophy of Science* I have employed Dedekind's postulate of a "cut." Here I shall follow a different procedure—and one widely accepted in contemporary mathematics.

Our starting point is the idea of *decimal notation* for numbers. This notation, as is well known, is based on powers of 10. The number 3527.418 thus means $3 \cdot 10^3 + 5 \cdot 10^2 + 2 \cdot 10 + 7 + 4/10 + 1/10^2 + 8/10^3$. In general, any number f, consisting of an integer z and a decimal fraction containing n digits after the decimal point, has the form $f = z + a_1 10^{-1} + a_2 10^{-2} + a_3 10^{-3} + \ldots + a_n 10^{-n}$ or, in abbreviation, $z.a_1a_2a_3 \ldots a_n$. Any fraction p/q whose divisor is some power of 10 can at once be written as a decimal fraction. Thus, $3.146 = 3 + 1/10 + 4/100 + 6/1000 = 3146/1000$. But if, after reduction to lowest terms, the fraction p/q has a denominator which is not a divisor of some power of 10, it cannot be represented as a decimal fraction with a finite number of decimal places. The fraction $1/3$ thus leads to the unending sequence 0.333333 . . ., and the fraction $1/7$ leads to the periodic sequence 0.142857142857142857 . . . . The periodicity of these decimal fractions (and 0.333333 . . . is also a "period") is no accident. On the contrary, it can be shown that if p/q is a rational number, its corresponding decimal expression *must* be periodic. The proof is simple. Decimal fractions are obtained by performing the elementary process of division. If at some stage in this process we

obtain a "zero" remainder, the decimal fraction is finite. If we obtain a "non-zero" remainder, it must be an integer between 1 and $q-1$, so that at each step there are not more than $q-1$ values possible for the remainder. Hence, if we carry on q divisions, there must be at least one remainder k which turns up a second time. But if that happens, all subsequent remainders must likewise reappear, and must reappear in the same order in which they first followed k. The decimal expression for any rational number is therefore either finite or it is periodic.

The converse is also true, for it can be shown that all periodic decimals are rational numbers. The field of rational numbers is thus equivalent to the domain of finite and periodic decimals.

If we now employ the notion of decimal numbers, we can extract square roots by a process of approximation. If we resort to this process in the case of $\sqrt{2}$, we obtain the following sequence:

$$1^2=1 < 2 < 2^2=4$$
$$(1.4)^2=1.96 < 2 < (1.5)^2=2.25$$
$$(1.41)^2=1.9881 < 2 < (1.42)^2=2.0264$$
$$(1.414)^2=1.999396 < 2 < (1.415)^2=2.002225$$
$$(1.4142)^2=1.99996164 < 2 < (1.4143)^2=2.00024449$$

and so on without end.

In this sequence we encounter no decimal number whose square is exactly equal to 2. And since the approximating decimals are formed by considerations other than those of elementary long division, they are not periodic. In other words, the attempt to extract $\sqrt{2}$ leads to an *infinite* and *non-periodic* decimal fraction. It therefore does not yield a rational number.

If we accept *infinite and non-periodic* decimals as "real" numbers, they must be regarded as *irrational*, i.e., as not statable as ratios.

If we now regard (as well we may) all finite decimals as special cases of infinite periodic decimals with "period" . . . 000000 . . ., we can speak of the *domain of real numbers* as the *totality of all infinite decimals*; and this domain includes all "rational" and "irrational" numbers. The periodic decimals are the rational numbers; the non-periodic decimals are the irrational numbers.

Our approach to the problem of irrational numbers based upon a sequence of approximations involving decimal fractions may, of course, be generalized in such a way that the approximations are no longer restricted to decimal intervals. The procedure then yields a sequence of "nested" intervals; and each sequence of such intervals

defines a number. A number, in other words, is now simply that "point" in the "dimension of quantity" toward which a sequence of "nested" intervals converges. And if the number, so defined, is not rational, then it is irrational.

Numbers have here lost their intimate contact with the sensory-intuitive basis of experience. They can no longer be "visualized" but are "intrinsically" defined by processes of construction. They *are*, in a sense, their processes of construction; and they are "*real*" only in so far as they are logically significant, free from contradiction, and "given" in the sequences of "nested" intervals which define them. They are *numbers* simply because the operations of addition, multiplication, subtraction, and division, and the five basic laws governing rational numbers are preserved throughout the extended domain. That is to say, the sequences of "nested" intervals are numbers because we can "operate" with them as with numbers. Thus, if a sequence of "nested" intervals defines an irrational number α, and another such sequence defines another irrational number β, we can obtain the sum $\alpha+\beta$ by adding, for the same interval, the "initial" values of the sequence defining β to the "initial" values of the sequence defining α, and the "end" values of the sequence defining β to the "end" values defining α. The "initial" and "end" values thus obtained define a new sequence of "nested" intervals, and this sequence is the sum of α and β.

It will be recalled that the demand for a solution of the equation $c=b-a$ when $a>b$ led to the introduction of *negative* numbers, that a similar demand for a solution for x of the equation $ax=b$ necessitated the introduction of *rational* numbers, and that, finally, the attempt to solve for x equations of the type $x^2=2$ brought us face to face with the problem of *irrational* numbers. It is evident, however, that our number system is not yet complete; for not all quadratic equations can be solved within the domain of real numbers. What, for example, is x when $x^2=-1$? It cannot be a real number because the square of a real number is never negative. Once more we are confronted with an alternative: Either the equation $x^2=-1$ remains unsolvable or our number system is still further extended by an inclusion of such numbers as will make the equation solvable. The ideal of a closed number system previously referred to leaves us no choice in the matter.

The new numbers are called "imaginary"—as distinguished from "real," i.e., as distinguished from the totality of decimals. The "imagi-

nary unit" i is defined by $i^2 = -1$; but, in general, an "imaginary" number is represented by any symbol $\sqrt[b]{n}$, where b stands for an even real number and n for any negative number. $\sqrt{-4}$, $\sqrt[4]{-2}$, and $\sqrt[8]{-3}$ are thus "imaginary" numbers.

The domain of "real" numbers constitutes a strictly defined one-dimensional continuum in which there is "nowhere no number" and in which every number corresponds to some uniquely definable "point." This continuum can be represented by a straight line. "Imaginary" numbers, on the other hand, cannot be located within this continuum. They lie in a different "dimension of quantity," and the "geometric" representation of a number system which includes "imaginary" numbers requires a "plane" rather than a "line." This means that, in principle, "imaginary" numbers have no longer any connection with the basic process of counting; that they are nothing but abstract symbols which are subject to the rule $x^2 = -1$. The question is, Does the introduction of these symbols really constitute a significant extension of the number system? And the answer to this question depends on whether or not we can "operate" with these symbols as with numbers.

If the elementary operations of addition, subtraction, multiplication, and division can be carried out with "imaginary" numbers, we are able to obtain such expressions as $-i$, $3i$, $3+\sqrt[6]{-4}$, $2+7i$, or, in general, $a+\sqrt[b]{n}$ or $a+bi$, where a and b are any two "real" numbers and n is negative; and operations with these symbols must conform to the five basic laws of arithmetic. The following equations, for instance, would have to be true: $(3+2i)+(1-5i)=(3+1)+(2-5)i$ $=4-3i$; and $(3+2i)(1+5i)=3+15i+2i+10i^2=(3-10)+(15+2)i$ $=-7+17i$.

Such results can be obtained by stipulating (1) that any symbol of the form $a+bi$ is to be regarded as a *complex number* with a *real* part a and an *imaginary* part b, and (2) that the operations of addition and multiplication shall be performable as if i were a real number, *except* that i^2 shall always be -1. We stipulate, in other words, that $(a+bi)+(c+di)=(a+c)+(b+d)i$, that $(a+bi)(c+di)=(ac-bd)$ $+(ad+bc)i$, and that $(a+bi)(a-bi)=a^2-abi+abi-b^2i^2=a^2+b^2$.

It is now not difficult to show (1) that the commutative, associative, and distributive laws hold, and (2) that subtraction and division involving "complex" numbers are also possible. Operations with "imaginary" numbers are therefore the same as operations with "real" numbers and are subject to the very same laws. The new domain of

numbers is thus free from inconsistencies, and it has this advantage: within it every quadratic equation of the form $ax^2+bx+c=0$ has a solution. For from $ax^2+bx+c=0$ we obtain

$$x^2+\frac{b}{a}x=-\frac{c}{a}$$

$$x^2+\frac{b}{a}x+\frac{b^2}{4a^2}=\frac{b^2}{4a^2}-\frac{c}{a}$$

$$\left(x+\frac{b}{2a}\right)^2=\frac{b^2-4ac}{4a^2}$$

$$x+\frac{b}{2a}=\frac{\pm\sqrt{b^2-4ac}}{2a}$$

$$x=\frac{-b\pm\sqrt{b^2-4ac}}{2a}$$

If $b^2-4ac \geqq 0$, then $\sqrt{b^2-4ac}$ is a "real" number and the solution of the equation is "real." If, on the other hand, $b^2-4ac < 0$, then $\sqrt{b^2-4ac}$ is an "imaginary" number and the solution is "complex." The domain of numbers, therefore, which is identical with the totality of all "real" *and* "complex" numbers, constitutes another *closed system* for all operations of addition, subtraction, multiplication, and division which we have so far considered. It is another mathematical *field* (cf. Courant and Robbins).

The further expansion of this field through an introduction of "hypercomplex" numbers, as encountered in factor analysis, we disregard here because of the difficulties of presentation and because it adds nothing that is new in principle to our discussion.

We began this discussion with a consideration of "counting" and, in general, with a consideration of the sensory-intuitive basis for positive integers or "natural" numbers, and for operations with "natural" numbers. So long as these operations were restricted to addition and multiplication, the system of numbers was *closed*; because every sum or product of two "natural" numbers is itself a "natural" number. However, recourse to subtraction as a mathematical operation necessitated the introduction of "zero" and of "negative" numbers, and recourse to division compelled the acceptance of "rational" numbers. The domain of "rational" numbers turned out to be a *closed* system or "field" for all operations of addition, subtraction, multipli-

cation, and division except that division by "zero" was excluded. However, as soon as the process of extracting roots was accepted as a legitimate mathematical operation, the system of "rational" numbers was no longer a closed system. In order to provide "roots" for every *positive* "rational" number, "irrational" numbers had to be introduced, and the number system was extended to include all "real" numbers. But even this was not sufficient. Since quadratic equations involving *negative* "rational" numbers were also to have a solution, "imaginary" and "complex" numbers had to be added in order to complete the domain of *all* numbers.

We were thus forced step by step to augment our original number system in order to satisfy the demand for a closed system for all operations. At every step we were guided, however, by the equally basic demand that the five fundamental laws of arithmetic be preserved throughout. The number system in its totality thus emerges as an integrated system under rigid control of basic laws—as the ideal of a self-sufficient, thoroughly logical, and all-comprehensive system; as the creation of man's constructive imagination and the result of his striving after integral unity. The *reality* of the numbers is grounded exclusively in the systemic requirements of the domain of all numbers and in the definitional stipulations which satisfy those requirements under the sway of the primary laws.

Limits, Functions, and the Calculus

We turn now to a consideration of certain concepts which are indispensable to broad fields of mathematics and which, at the same time, illustrate in specific ways the nature and function of mathematical operations. Foremost among these concepts is that of *limits*.

If we arrange all positive integers in their "natural" order, we obtain the *sequence* 1, 2, 3, 4, . . ., where the dots mean "and so on." Since the sequence "goes on" beyond any particular number n which we may select, the sequence is *unending* and, in this sense, *infinite*. Other unending or infinite sequences may readily be formed. All of them have the form $x_1, x_2, x_3, \ldots, x_n, \ldots$, where the subscript indicates the ordinal place of x in the sequence. x_1 thus refers to the first position or first "term," x_2 to the second, x_3 to the third, and x_n to *any* position or "term" we may choose. x_n, in other words, is the *general term* of the sequence.

The general term of any given sequence is of special significance, for, if specifically defined, it is the *law* which governs the construction

of the sequence. Thus, if the general term is $\frac{n}{n+1}$, the sequence is $1/2, 2/3, 3/4, 4/5, \ldots, \frac{n}{n+1}, \ldots$, and if the general term is $1-\frac{1}{10^n}$, the sequence is $.9, .99, .999, .9999, \ldots, 1-\frac{1}{10^n}, \ldots$. The general term $+1$ is the "law" governing the sequence of positive integers $1, 2, 3, 4, \ldots, +1_n, \ldots$. The term $\frac{1}{2^n}$ gives rise to the sequence $\frac{1}{2^1}, \frac{1}{2^2}, \frac{1}{2^3}, \frac{1}{2^4}, \ldots, \frac{1}{2^n} \ldots$; and such general terms as $\frac{1}{n}, \frac{(-1)^n}{n}$; $\frac{(-1)^{n+1}}{10^n}$, and $\frac{2^{n-1}}{2^n}$ define still other sequences. They are the "laws" which determine in each case what shall and what shall not be an element of the sequence.

Let us assume now that the general term $2-\frac{1}{10^n}$ is the "law" of a sequence. That is to say, let us assume as given the sequence 1.9, 1.99, 1.999, 1.9999, . . .; $2-\frac{1}{10^n}, \ldots$. It is evident that, as n increases, the difference between 2 and $2-\frac{1}{10^n}$ decreases; and since n can be increased indefinitely, the difference between 2 and $2-\frac{1}{10^n}$ can be decreased indefinitely. By making n great enough, we can come as close as we please to 2. The number 2, in other words, is the *limit* which the sequence approaches as n "tends to infinity." In a similar way, the general term $\frac{1}{n}$ determines a sequence whose *limit* is 0, i.e., it determines a sequence which makes it possible for us to come as close to 0 as we please; for $\frac{1}{n}$ determines the sequence $\frac{1}{1}, \frac{1}{2}, \frac{1}{3}, \frac{1}{4}, \ldots, \frac{1}{n}, \ldots$, and as n approaches ∞, $\frac{1}{n}$ approaches 0.

However, the phrase, "we can come as close as we please," is somewhat vague and ambiguous. What the mathematician means is this: If we choose any "degree of closeness" to the limit of a sequence, then we can find a place in the sequence such that every term follow-

ing this new place is closer to the limit than is the originally chosen "degree." In more technical language, the number a is the *limit* of a sequence $x_1, x_2, x_3, \ldots, x_n, \ldots$ if, given any positive number ϵ, no matter how small, there exists a term x_N of the sequence such that for every succeeding term x_m (with $m > N$) the distance $|x_m - a|$ of every x_m from a is less than the preassigned value of ϵ.

If a sequence a_n has the limit a, we write symbolically $\lim a_n = a$ as $n \rightarrow \infty$, or $a_n \rightarrow a$ as $n \rightarrow \infty$; i.e., a_n converges to a as n tends to infinity. Both "approach" and "tending to" must be understood in their joint logical significance of a law of progression rather than in some "dynamic" sense of intuitive processes. The statement "$a_n \rightarrow a$ as $n \rightarrow \infty$" is only a symbolic way of saying that for every ϵ a margin $|x_m - a| < \epsilon$ can be found; and no part of this statement, taken by itself, has any meaning.

The limit of a sequence need not be a term of the sequence, i.e., we may have $a_n \rightarrow a$ even though none of the numbers x_n in the sequence is equal to a. This is so because n "tends to," but does not actually *assume*, the value ∞.

We introduce next the idea of *function*. An expression such as x^2+4x-5 has no definite numerical value until a specific value has been assigned to x. The value of the expression, in other words, is a *function* of the value of x; i.e., $x^2+4x-5=f(x)$. If we assume $x=3$, then $3^2+4\times 3-5=16$, and, therefore, $f(x)=16$. In a similar manner the value of $f(x)$ may be obtained by making specific substitutions for x in other mathematical expressions. That is to say, the value of $f(x)$ may be obtained by substituting constants for the variables of a given expression.

If the variable is a "real" number ranging over a specified interval $a \leqq x \leqq b$, x is called a *continuous* variable in the interval, and the points of the interval form the "domain of variability" of x. This "domain of variability" may be co-extensive with the domain of all "real" numbers. If with each value of a variable x there is associated a definitely determined value of some other variable u, then u is a *function* of x. Thus, for example, $u=f(x)$ when $u=x^2$; or $u=f(x)$ when $u=\frac{1}{1+x^2}$. A mathematical function is thus a "law" governing the interdependence of variable quantities.

The laws of the empirical sciences, as expressed in mathematical equations, are, in this sense, *functional expressions*; for they specify how certain quantities (the "dependent" variables) depend on others

(the "independent" variables) when the latter are made to vary in conformity with experimental findings or with assumed conditions. Thus the distance covered by a freely falling body is $f(\frac{1}{2}gt^2)$, and the maximum height reached by a projectile which is fired at an angle to the horizontal is $f\frac{(v^2 \sin^2\theta)}{2g}$. Functions so understood must not be confused with the traditional idea of "cause" and "effect" relationships; for the empirical laws statable as functions are "reversible." We shall return to this point in Chapter VIII.

Let us assume now that a sequence of values of x approaches a as a limit, i.e., let us suppose that we have $x_1, x_2, x_3, \ldots, x_n, \ldots \rightarrow a$. Let us suppose, furthermore, that every term x_n of this sequence contains an independent variable x for some specific function $f(x)$. In other words, $f(x)$ has been defined. It may then be the case that the sequence $f(x_1), f(x_2), f(x_3), \ldots, f(x_n), \ldots$ approaches a limit L. Now, if the same values of x are elements in another series so that $x_1', x_2', x_3', \ldots, x_n' \ldots \rightarrow a$, then a second sequence of functions of x, i.e., $f(x_1'), f(x_2'), f(x_3'), \ldots, f(x_n'), \ldots$ may also converge to L, or it may converge to a new limit $L' \neq L$, or it may not converge at all. If for *every* sequence, $x_1, x_2, x_3, \ldots, x_n, \ldots \rightarrow a$, all x_n's being in the "domain of variability" of x and $f(x_1), f(x_2), f(x_3), \ldots, f(x_n), \ldots \rightarrow L$, then L is the limit of $f(x)$ as x approaches a. That is to say, $f(x) \rightarrow L$ as $x \rightarrow a$. If $f(a)$ is defined, if $\lim_{x \rightarrow a} f(x)$ exists, and if $\lim_{x \rightarrow a} f(x) = f(a)$, then the function is said to be *continuous* at $x = a$. If these conditions are not fulfilled, the function is discontinuous at $x = a$.

The combination of limits and functions provides a powerful instrument for mathematical analysis. Consider, for example, the following case: Let y be a function of x at x_1 and let $y_1 = f(x_1)$. This equation defines the point P_1 in Fig. 1. Let P_2 be defined by $y_2 = f(x_2)$. P_1P_2 is then a secant to the given curve. The slope of this secant is $\frac{y_2 - y_1}{x_2 - x_1}$. $x_2 - x_1$ is the *difference* or *change* in x, symbolized by Δx; $y_2 - y_1$ is the *change in* y, symbolized by Δy. Making the appropriate substitutions, the slope of the secant is $\frac{\Delta y}{\Delta x}$. As P_2 approaches P_1, x_2 approaches x_1 and $\Delta x \rightarrow 0$. At the same time, y_2 approaches y_1 and $\Delta y \rightarrow 0$. In other words, $\frac{\Delta y}{\Delta x}$ approaches a limit. If this limit exists, it is the slope of the tangent at x_1, y_1.

Let us suppose now that a curve is given so that $y=x^2$ (Fig. 2). We desire to find the slope of the tangent at $x_1=3$. In this case, $y_1=9$. We now select a point $x_2=3+\Delta x$. For this value of x, $y_2=(3+\Delta x)^2=9+6\,\Delta x+(\Delta x)^2$. Since Δy is the difference between y_2 and y_1, $\Delta y=6\,\Delta x+(\Delta x)^2$. Division of both sides of this equation by Δx yields the slope of the secant as $\frac{\Delta y}{\Delta x}=6+\Delta x$. As Δx approaches 0, the actual slope of the tangent at (3, 9) is $\lim_{\Delta x\to 0}\frac{\Delta y}{\Delta x}=6$.

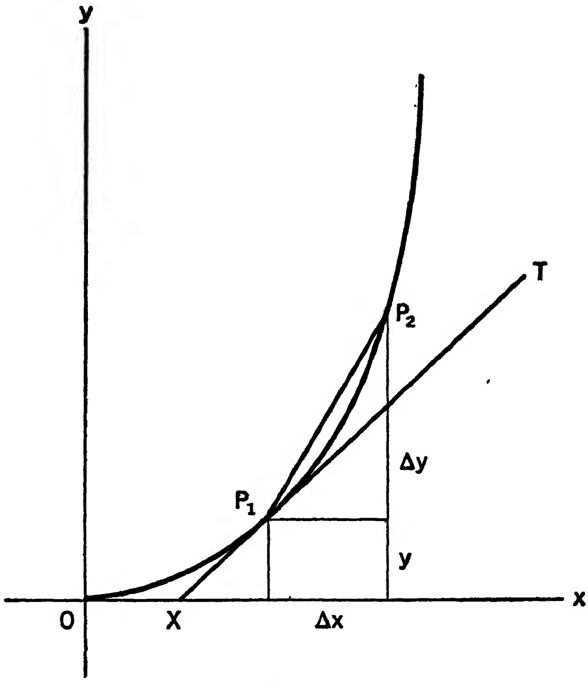


Fig. 1

It may be supposed that in this demonstration $\Delta y=f(x+\Delta x)-f(x)$. The "differential quotient" $\frac{y_2-y_1}{x_2-x_1}=\frac{\Delta y}{\Delta x}$ is therefore a new or *derivative* function of $f(x)$. We denote it by $\frac{dy}{dx}$, "the derivative of y with respect to x," and write: $\frac{dy}{dx}=\lim_{\Delta x\to 0}\frac{\Delta y}{\Delta x}$. This expres-

sion, however, must not be interpreted as meaning that dx is $\lim_{\Delta x \to 0} \Delta(x)$, or that dy is $\lim_{\Delta \to 0} \Delta(y)$; for each of these limits *is* 0, and $\frac{dy}{dx}$ would then be $\frac{0}{0}$, which is meaningless. What makes the expression $\frac{dy}{dx}$ mathematically so significant is the fact that two functions may each approach 0 while their quotient approaches a value other than 0. For instance,

$$\lim_{h \to 0} (4h) = 0 \text{ and } \lim_{h \to 0} (2h) = 0, \text{ but } \lim_{h \to 0} \left(\frac{4h}{2h}\right) = \frac{4}{2} = 2.$$

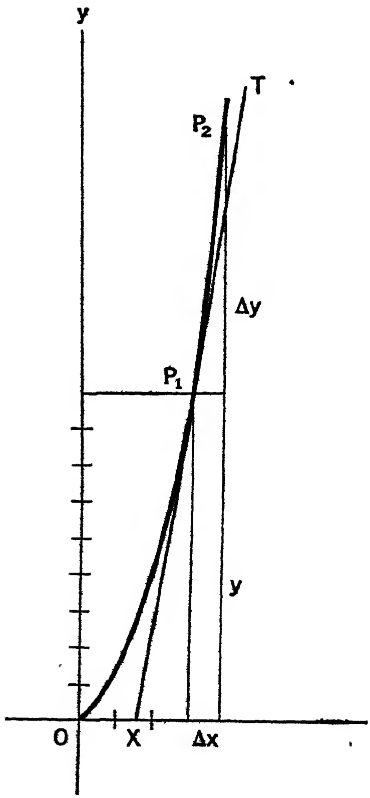


FIG. 2

Finding the "derivative" of a given function is the problem dealt with in *differential calculus*; and the differential calculus is one of the most powerful tools in dealing mathematically with physical phenomena. It can be readily shown why this is so.

Let us suppose that an automobile travels 160 miles in 4 hours, going in a straight line. Its average "velocity" is $\frac{160}{4}$ or 40 m.p.h. We can express this in the following way: If "distance" is represented by y and "time" by x, then the *specific* distance of 160 miles (as the automobile moves from y_1 to y_2) is represented by $y_2 - y_1$; and the specific time of 4 hours (as the clock moves from the beginning of the interval x_1 to its end x_2) is $x_2 - x_1$. The average rate of change is therefore $\frac{y_2 - y_1}{x_2 - x_1}$ or $\frac{\Delta y}{\Delta x}$; i.e., it corresponds graphically to the slope of the secant.

If y varies with x at a *uniform rate*, then the rate of change at any

instant is the same as the average rate for the whole interval. But let us suppose that y varies with x but not at the same rate; i.e., let us suppose, for example, that we deal not with the motion of an automobile which is being driven at a uniform rate of speed in a straight line, but with the motion of a freely falling body. What is the speed with which the body falls at any particular moment? What, in other words, is the rate of change of y at any given instant of x?

If, beginning with the "given instant," we select a sufficiently small interval of time, the average speed for this interval comes close to the instantaneous speed at the intended instant. This suggests a solution of the problem through the employment of limits; for we find that, if $y=f(x)$, then the instantaneous rate of change of y with respect to x_1, at $x=x_1$, is $\lim\limits_{x \to 0} \frac{\Delta y}{\Delta x} = \frac{dy}{dx}$; i.e., it corresponds graphically to the slope of a tangent. The same mathematical devices, there-

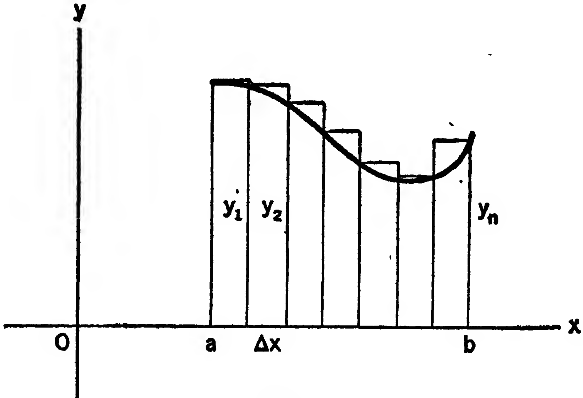


Fig. 3

fore, which provide solutions for the problems of secants and tangents thus provide solutions also for problems of motion and of changes of motion; and, in general, the same mathematical devices which provide solutions for problems concerning the nature and qualities of "curves" provide solutions for all physical phenomena which can be graphically represented by curves and the qualities of curves.

Let us now consider a different situation. Let $y=f(x)$ determine a curve (Fig. 3). What is the area A "under the curve" as bounded by a and b? If we divide the interval from a to b on the x-axis into n equal parts of length Δx, then $n \cdot \Delta x = b - a$. If $y_1, y_2, y_3, \ldots, y_n$

are the values of y at the points of division of $b-a$, we obtain the "staircase" area indicated in Fig. 3, whose area is

$$S = y_1 \cdot \Delta x + y_2 \cdot \Delta x + y_3 \cdot \Delta x + \ldots + y_n \cdot \Delta x.$$

This sum is obviously an approximation of the intended area under the curve. It approaches the intended area as Δx is made smaller and smaller, i.e., as n (the number of times Δx is contained in $b-a$) increases indefinitely. Since $\Delta x = \frac{b-a}{n}$, $\Delta x \rightarrow 0$ as $n \rightarrow \infty$. We thus find that

$$\text{area } A = \lim_{\substack{\Delta x \rightarrow 0 \\ n \rightarrow \infty}} (y_1 \cdot \Delta x + y_2 \cdot \Delta x + \ldots + y_n \cdot \Delta x).$$

This limit is called the *definite integral* of $f(x)$ from a to b, and is usually written as

$$\int_a^b f(x)\,dx,$$

where a and b are the "limits of integration."

The definite integral as the limit of a sum of small quantities is the central concept of the *integral calculus*. Its applications in the realm of the empirical sciences are legion, but we are not concerned with them here.

Another problem is, however, of considerable interest. It is this: Leibniz, proceeding from the quotient of a difference, $\frac{y_2 - y_1}{x_2 - x_1} = \frac{\Delta y}{\Delta x}$, regarded the "differential quotient," $\frac{\Delta y}{\Delta x} = \frac{dy}{dx}$ for $\Delta y \rightarrow 0$ and $\Delta x \rightarrow 0$, as an "infinitely small quantity" and spoke of the integral as the sum of infinitely many "infinitely small quantities" $f(x)dx$. Although he said ("Essais de Théodicée," *Schriften* VI, p. 90) that the infinitely small quantities must be understood as the "state of disappearance" or the "state of beginning" of a quantity, he also wrote (*Mémoire pour l'histoire des sciences et des beaux arts*—Letter to Pater Tournemine, October 28, 1714) that instead of regarding the "infinitesimal quantities" as zero—as did Fermat, Descartes, and Newton—it is necessary to suppose that they are "something." They are misunderstood if they are "taken as zero."

These remarks gave rise to the mystic interpretations of the calculus which we encounter so frequently in the writings of Leibniz's successors, and which led Bishop Berkeley to the repudiation of the calculus.

All mystery disappears, however, if we discard the idea of "infi-

nitely small quantities" and define the "differential" as the *limit* of $n \to \infty$, $\Delta x \to 0$, or of some other $\underset{\to}{\lim}$, and define the "integral" as the *limit* of a finite sum. Through such definitions, all "metaphysical" elements are excluded from the realm of calculus and mathematics is kept self-consistent and intrinsically complete. It remains a system of interrelated concepts under exclusive determination through laws. Notions which are extraneous to such a system and which are not rigidly definable in terms of mathematical procedures can have no bearing upon any aspect of the mathematician's work.

Infinity and Transfinite Numbers

In a previous section we said that the sequence of positive integers 1, 2, 3, 4, . . . is unending and, in this sense, infinite. That this is true is obvious when we keep in mind that the whole sequence is generated through successive applications of the operation "$+1$," and that there is no intrinsic reason why this operation should not be performable at some given place in the sequence. No matter how large an integer n we select, we can always obtain a still larger one by performing $n+1$.

But to say that a sequence is "infinite," in the sense of "being without end," is one thing, and to speak of "infinity," in the sense of a "completed totality," is something quite different; for in the former case we mean that a certain generic operation can be performed indefinitely, but in the latter case we speak in a manner which seems to imply that the infinitely many steps in an unending sequence have actually all been completed. If this were really our meaning, the notion of infinity would be self-contradictory and therefore useless in mathematics. What, then, is meant by 'infinity'? A comprehensive answer to this question has been provided in the modern theory of "sets" or "classes" which, in its origins, goes back to the work of Georg Cantor.

The point of departure for this theory is the idea of an *aggregate* or set, or of what Cantor himself called a *Menge*. In its mathematical significance such a *set* or *Menge* is *any collection of objects defined by some rule which rigidly determines which objects belong to the collection and which do not.* Any sequence which is defined by a "general term" (as discussed in the preceding section) is thus a set. But the point to be noted especially is that only objects which are completely determined by the rule that defines the set can belong to the collection;

for this stipulation implies that, *if we know the rule,* we know, *in principle, all* members of the set *without having investigated each possible member individually.* Hence, if the rule defines an infinite set, we can speak of this set as being an "infinite totality" without having achieved the impossible, i.e., without having completed the unending sequence of steps required in a complete enumeration. The rule itself, since it *generates* the set, *is* in a very precise sense the set itself. When the *rule* is known, the nature of *all* elements is known as definitely as if each element had been examined individually. Reference to an "infinite totality" is therefore essentially a reference to some rule defining that "totality," or, if you prefer, to the totality of elements defined by some rule.

Is it possible to operate with "infinities" as here defined? The answer is Yes; but some of the operations which are possible within the closed system of complex numbers may have to be redefined.

We can compare the "magnitude" of a set A with the "magnitude" of another set B by putting the elements of A in one-to-one correspondence with the elements of B. If to each element of A there corresponds one, and only one, element of B, the correspondence is *biunique* and the sets A and B are *equivalent.* Since the elements of two *finite* sets can be put in one-to-one correspondence only if both sets have the same number of elements, equivalence, as just defined, coincides with *equality of number* and pertains to the *cardinal* meaning of numbers.

If every element of a set A′ is also an element of a set A, then A′ is said to be a subset of A. This includes the limiting cases of A′=A, and A′=0. If A contains elements which are not contained in A′, then A′ is a proper subset of A. If a set is *finite,* then it cannot be equivalent to any one of its proper subsets, for a proper subset can contain at most only $n-1$ elements, where n is the number of elements in the finite set. This means that, as far as the finite sets are concerned, a "part" is never equal to the "whole." But if a set is *infinite,* then it can readily be shown that any one of its proper subsets is equivalent to the set itself. In other words, it can be shown that, as far as infinite sets are concerned, a "part" is equivalent to the "whole," and that a principle of reasoning which is perfectly valid so long as we deal with finite sets breaks down when we transcend finitude. An example will illustrate the point:

If A is the set of all positive integers, and if A′ is the subset of all

odd numbers, then A′ is a proper subset of A and yet is equivalent to A; for

$$\begin{array}{ll} A & 1,\ 2,\ 3,\ 4,\ 5,\ \ldots \\ \\ A' & 1,\ 3,\ 5,\ 7,\ 9,\ \ldots \end{array}$$

where the double-headed arrows between the elements of the two sets indicate a complete one-to-one correspondence.

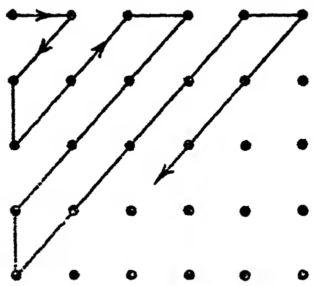
Making use of the relationships here indicated, we now define an infinite or "transfinite" set as any set A for which there exists a proper subset A′ which is equivalent to A.

If a set is equivalent to the set of all natural numbers, we call it *denumerable.* A denumerable set is necessarily "infinite" because the set of all natural numbers is infinite; but not all "infinite" sets are denumerable. The set of all rational numbers, for example, is denumerable and infinite. That it is denumerable may be shown in the following manner: Since all rational numbers can be written in the form *a/b*, where *a* and *b* are integers, we can arrange the set of all rational numbers thus:

$$\begin{array}{cccccccc} 1 & 2 & 3 & 4 & 5 & 6 & 7 & \ldots \\ \frac{1}{2} & \frac{2}{2} & \frac{3}{2} & \frac{4}{2} & \frac{5}{2} & \frac{6}{2} & \frac{7}{2} & \ldots \\ \frac{1}{3} & \frac{2}{3} & \frac{3}{3} & \frac{4}{3} & \frac{5}{3} & \frac{6}{3} & \frac{7}{3} & \ldots \\ \frac{1}{4} & \frac{2}{4} & \frac{3}{4} & \frac{4}{4} & \frac{5}{4} & \frac{6}{4} & \frac{7}{4} & \ldots \\ \frac{1}{5} & \frac{2}{5} & \frac{3}{5} & \frac{4}{5} & \frac{5}{5} & \frac{6}{5} & \frac{7}{5} & \ldots \\ \frac{1}{6} & \frac{2}{6} & \frac{3}{6} & \frac{4}{6} & \frac{5}{6} & \frac{6}{6} & \frac{7}{6} & \ldots \end{array}$$

This scheme indicates at once that the set in question is infinite. If we

now connect the individual numbers by a broken line, thus



we form the sequence 1, 2, 1/2, 1/3, 2/2, 3, 4, 3/2, 2/3, 1/4, 1/5, 2/4, 3/3, 4/2, 5, 6, 5/2, 4/3, ... If we cancel all numbers for which a and b have a common factor, so that each rational number is reduced to its simplest form and therefore appears only once in the whole sequence, we obtain the set 1, 2, 1/2, 1/3, 3, 4, 3/2, 2/3, 1/4, 1/5, 5, 6, 5/2, 4/3, . . .; and since this set, as is obvious, can be put in one-to-one correspondence with the set of all natural numbers, it follows that the set of all *rational* numbers is denumerable.

The set of all *real* numbers, however, is not denumerable. Consider, for example, the set A of all real numbers between 0 and 1, including 1. This set, as will be remembered from previous discussions, is identical with the set of all infinite decimal fractions between 0 and 1. And as a set of these fractions the set A is *not* denumerable. Cantor's "diagonal procedure" proves this fact; for this procedure enables us to show that if we have arranged all real numbers in an "infinite" table of infinite decimal fractions, thus

$$\begin{array}{ll}
1) & 0.a_1a_2a_3a_4a_5a_6 \ldots \\
2) & 0.b_1b_2b_3b_4b_5b_6 \ldots \\
3) & 0.c_1c_2c_3c_4c_5c_6 \ldots \\
4) & 0.d_1d_2d_3d_4d_5d_6 \ldots \\
5) & 0.e_1e_2e_3e_4e_5e_6 \ldots \\
6) & \ldots\ldots\ldots\ldots \ldots
\end{array}$$

(where $a_1a_2a_3 \ldots$; $b_1b_2b_3 \ldots$, etc., are ciphers of the series 0, 1, 2, 3, . . ., 8, 9), we can always find another infinite decimal fraction between 0 and 1 which is not an element of the set, namely, the

decimal fraction $0.\alpha_1\beta_2\gamma_3\delta_4 \ldots$ (where α_1 differs from a_1, β_2 differs from b_2, γ_3 differs from c_3, and so on; so that this decimal fraction as a whole differs from every fraction in the original set but is, nevertheless, a real number between 0 and 1). The set of all real numbers between 0 and 1 is thus not denumerable. And since the same can be shown for the set of all real numbers between 1 and 2, between 2 and 3, between 3 and 4, and so on for all intervals of the dimension of real numbers, it follows that the set of *all* real numbers also is *not* denumerable.

If this is so, then we must admit logical distinctions within the infinite; for at least two different types of infinite sets have already been pointed out: the denumerable and the not denumerable sets.

We now employ the idea of "cardinal number" and stipulate that if two sets, A and B—be they finite or infinite—are equivalent, then they have *the same cardinal number*. And we stipulate, furthermore, that if a set A is equivalent to a subset of B, while B is not equivalent to A or to any of its subsets, then the set B has a *greater cardinal number* than set A. This stipulation is manifestly true for all finite sets. If it is assumed to be true for infinite sets also, then it follows that the cardinal number of a set which is not denumerable is not only different from, but greater than, the cardinal number of a denumerable set. The distinctions within the infinite, in other words, are *quantitative* distinctions, *distinctions of magnitudes*.

By various ingenious devices Cantor could show the possibility of deriving infinitely many infinite sets which are not equivalent. That is, he could show that infinitely many quantitative differentiations are possible within the infinite or, what amounts to the same thing, that an infinite sequence of transfinite cardinal numbers can be constructed. The first, or lowest, of these numbers is the cardinal number of all denumerable sets; it is symbolized by a. Another such number is the cardinal number of the continuum, symbolized by c. A third is the cardinal number of all univocal real functions, symbolized by f. Since the set S of the subsets of any given set A always possesses a cardinal number greater than A, there is no end to the cardinal numbers which lie beyond f; for no matter what set A is given, we can always form one having a cardinal number greater than A by forming the set S of all subsets of A.[1]

[1] The process may be illustrated in the following manner: Let the set A consist of the three integers 1, 2, 3. Its cardinal number is 3. But the subsets of A are, respectively, (1,2,3), (1,2), (1,3), (2,3), (1), (2), (3), and (0). The set S of all the subsets, therefore, has the cardinal number 8.

Transfinite numbers can be added together and multiplied, and can be "raised to power" ad infinitum. That is to say, mathematical operations with these numbers are possible. The details of these operations need not concern us here (cf. Werkmeister).

When Cantor's theory of sets was first developed, it was severely criticized by philosophers and mathematicians alike. Some of the criticisms were irrelevant and involved a complete misunderstanding of Cantor's intentions and of his achievement. Other criticisms, however, were important both for mathematical analyses and for philosophical evaluations. These criticisms centered around the "non-constructive character" of Cantor's reasoning, and around the "paradoxes" entailed by the theory. They were criticisms, in other words, which tended to cast doubt upon the whole procedure involving transfinite numbers, and gave rise to a somewhat stormy controversy concerning the very foundations of mathematics and of mathematical proof. In this controversy, Brouwer and his fellow "finitists" vigorously opposed Hilbert and the "formalists."

The objection to the "non-constructive character" of Cantor's reasoning is essentially an objection to the employment of *indirect* proofs in mathematics, i.e., it is an objection to proofs which, instead of leading to an actual "construction" of what is in question, "demonstrate" the truth of a theorem A by showing that its contradictory A′ is absurd. The force of the "non-constructive" or indirect proof rests entirely upon the validity of the principle of the excluded middle according to which the falsity of A′ necessarily establishes the truth of its contradictory A.

There is, of course, an undeniable difference between solving a problem by actually "constructing" a tangible solution, and solving it by showing that if no solution existed we should be forced into a contradiction. In the field of mathematics, however, both types of proof have often been employed. In the realm of finitude, the indirect proofs often have the advantage of simplicity. They constitute a convenient supplement to direct or "constructive" proof. In the realm of the infinite, "constructive" or direct proofs are, as a rule, impossible because we cannot perform infinite operations any more than we can perform an infinite number of finite operations. For example, the proof given above that the set of all real numbers is not a denumerable set is essentially a "non-constructive" or indirect proof; for we start with the assumption that the set is denumerable and show that this assumption is contradicted by the fact that at least one real num-

ber can be found which is not included in the original set but which is clearly an element of the set.

The "finitists" or "intuitionists" in mathematics, under the leadership of Brouwer, insist that "non-constructive" proofs are inadmissible and that all phases of mathematics which depend upon such proofs should be discarded. The "formalists," on the other hand, argue that "non-constructive" proofs are reliable and that too much of mathematical knowledge would have to be sacrificed if all demonstrations depending upon such proofs were to be disregarded.

The "paradoxes" entailed by Cantor's theory of sets gave rise to a still different group of problems. If the formation of sets remains unrestricted, then we can form some sets which do not contain themselves as elements, and we can form other sets which do contain themselves as elements. Let us suppose we form a set A of *all* and *only* those sets which do not contain themselves. Does A contain itself? If it does not contain itself, then it *must* contain itself; for, by stipulation, A is the set of *all* sets which do not contain themselves. But if A contains itself, then it *cannot* contain itself; for, by stipulation, A contains *only* sets which do not contain themselves. But A either contains itself or it does not contain itself. Whichever alternative we accept, we encounter a contradiction.

If the consequences of a theory are contradictory, can the theory itself be accepted as sound? More specifically, if mathematical "existence," as has often been pointed out, is equivalent to "freedom from contradiction," is not the whole range of that "existence" in doubt until mathematics itself has been proved to be free from contradictions? Considerations of this type led Russell, Hilbert, and others to a re-examination of the foundations of mathematics. Russell hoped to find the solution in broader logical interpretations; Hilbert attempted a direct proof of consistency. But neither the "logical" approach of Russell nor the "formalistic" approach of Hilbert could succeed without recourse to the intuitive-synthetic realm of first-person experience. We shall return to this point after a brief discussion of a still different field of mathematics.

Geometries

Our discussions have so far been concerned with numbers and with operations involving numbers. The field of geometry has been neglected. This field, however, deserves special attention because in it the postulational nature of mathematics is most readily demonstrable.

It is true, of course, that geometry does not constitute a domain of mathematics which is completely detached from numbers and operations with numbers; for geometry can be arithmetized. That is to say, every geometrical object and every geometrical operation can be represented in the realm of numbers. Through the introduction of "coordinates" expressions in terms of numbers can be found which completely characterize geometrical objects; and operations with numbers thus assigned correspond to operations with the geometrical objects themselves.

If we assume a system of "Cartesian coordinates" x and y (straight lines intersecting at right angles), then a "point" is defined through the numerical values of x, y, and the distance d between two points, P_1 (defined by x_1, y_1) and P_2 (defined by x_2, y_2), is determined by the equation $d^2=(x_1-x_2)^2+(y_1-y_2)^2$, which is the algebraic expression for the Pythagorean Theorem.

If, in the same system of coordinates, C is a fixed point having coordinates $x=a$ and $y=b$, then the *locus of all points P* with distance r from C is a *circle* of radius r around C as center. Making the appropriate substitutions in the equation determining distance, the circle, as just defined, is fully determined by the "*equation of the circle*": $(x-a)^2+(y-b)^2=r^2$.

In a similar manner, the *ellipse* is completely determined by $\frac{x^2}{p^2}+\frac{y^2}{q^2}=1$, and the *hyperbola* by $\frac{x^2}{p^2}-\frac{y^2}{q^2}=1$. A straight line is characterized by $ax+by=c$, where a, b, and c are fixed constants. And the point of intersection of two straight lines is obtained when the solution for x, y of the equations defining the two lines, $ax+by=c$ and $a'x+b'y=c'$, is taken as the coordinates of the point. The points of intersection of other lines are found by a similar operation involving the respective equations of the lines in question.

Enough has been said, I believe, to show that, through the procedures of *analytic geometry*, the whole field of geometry can be fused with the field of numbers. There is, however, another aspect to geometry which concerns us here.

When Euclid developed elementary plane geometry as a "deductive science," he proceeded in a manner which has become typical of all postulational-deductive sciences. He *defined* a small number of basic terms (such as 'point,' 'straight line,' and 'plane'), and then selected five *postulates* which stated the conditions or relations pertaining to

these terms. The rest of the system, i.e., the geometrical *theorems*, he deduced from these definitions and postulates.

It is evident that in such a system, if it is logically rigorous, the truth of the theorems depends upon the truth of the postulates. The selection of postulates is therefore exceedingly important. Euclid himself seems to have been fully aware of this fact, for he selected postulates which, in his judgment, were *self-evident* and beyond dispute. Among his postulates were the following four: (1) It shall be possible to draw a straight line joining any two points. (2) A terminated straight line may be extended without limit in either direction. (3) It shall be possible to draw a circle with given center and through a given point. (4) All right angles are equal. Most people would accept these postulates as "self-evident" even today.

Euclid's fifth postulate is, however, of a somewhat different nature, and Euclid himself seems to have had some doubts about it, for he did not employ it in a number of proofs when recourse to it would have greatly facilitated his demonstrations. In Heath's translation this fifth postulate reads: "If two straight lines in a plane meet another straight line in the plane so that the sum of the interior angles on the same side of the latter straight line is less than two right angles, then the two straight lines will meet on that side of the latter straight line."

Nobody who understands the meaning of this postulate, as given in Euclid's terminology, can doubt its "truth." Its complexity, however, suggests that it might be derivable from the other postulates; i.e., that it is not a postulate at all but a theorem. For centuries the keenest minds of Europe attempted to solve this "riddle"; but the only positive result of the early investigations seems to have been the discovery that Euclid's formulation of the fifth postulate can be replaced by a statement to the effect that "to a given straight line one and only one parallel line can be drawn through a point not on that line." This "parallel" postulate is completely equivalent to Euclid's own formulation. If the latter is accepted as postulate, the "parallel" statement can be deduced as a theorem; and if the "parallel" statement is accepted as a postulate, Euclid's statement follows from it as a theorem. In either case the rest of the system of geometry remains unaffected.

During the nineteenth century, Lobatschewsky, Bolyai, and Riemann demonstrated that complete and consistent systems of geometry can be developed even if Euclid's fifth postulate is discarded and if a stipulation is put in its place which contradicts the Euclidean asser-

tion that *one and only one* line can be drawn parallel to a given straight line and through a point not on that line. To be sure, the theorems derived from the "non-Euclidean" set of postulates differ in some respects from the theorems of Euclidean plane geometry. For example, if we assume, with Bolyai, that *more than one* line may be drawn through a given point and parallel to a given straight line, we find that the sum of the angles in a triangle is always *less* than 180° and that it *varies* with the size of the triangle; and if we assume, with Riemann, that *not even one* line can be drawn through a given point and parallel to a given straight line, we discover that the sum of the angles in a triangle is always *more* than 180°. But Cayley and Klein could prove that there is nothing inconsistent in the various "non-Euclidean" geometries and that all are derived with logical rigor from the modified sets of postulates.

Following Klein's method we can construct a Euclidean "model" and can then *re-name* some of the Euclidean objects and relations in such a way as to create a non-Euclidean geometry. Since the latter thus presents to us the same objects and relations as does the Euclidean geometry, but does so under different names, it must be at least as consistent as is the Euclidean "model" from which it is derived. If, for example, we have a sphere as our "model," we can call the surface of the sphere a "plane"; and, following the suggestion that a "straight line" is the shortest distance between two points in a plane, we can call the great circle of this sphere the "straight line." We shall then find that the "new" geometry is identical with the geometry of Riemann. No parallel lines are possible, for all "straight lines" (great circles) intersect; and the sum of the angles in a triangle is greater than 180°, for the sides of each triangle are "straight lines" which are, in the Euclidean sense, sections of the great circle. There is thus a one-to-one correspondence between Riemannian "non-Euclidean" geometry and the Euclidean geometry of spherical surfaces, and the former is therefore as consistent within itself as is the latter (from which it is "derivable" by the simple process of re-naming "spherical surface," "great circle," etc.).

If logical consistency is a criterion of mathematical "reality," then the new geometries are as "real" as is the Euclidean version. All of them are relational structures logically contrived and derivable from certain definitions and assumptions; and as such structures they have their own validity and their own "truth."

So long as physicists accepted Newtonian mechanics as an adequate

interpretation of all motions, Euclid's system of geometry seemed to be in a favorable position as being the system which is actually descriptive of physical space. But in a world in which the principle of relativity gives us a more comprehensive understanding of motions and systems of reference, a modified Riemannian geometry rather than the classical system of Euclid provides the best interpretation of space, and Euclidean geometry has lost much of its former significance.

Logically, the various types of geometry are all on an equal footing. Experientially, Euclidean geometry has the advantage of simplicity so long as we deal with relatively small spaces and slow motions; but a modified Riemannian form has the advantage of greater flexibility and comprehensiveness whenever we consider cosmic dimensions and velocities approaching that of light.

The empirical significance of the various geometries does not concern us at the moment; their logical structure, however, is all-important, for an analysis of this structure leads from a new angle to the broad questions concerning the foundations of mathematics and concerning the nature of mathematical reasoning and mathematical validity. And to these questions we turn now.

Postulational Methods

We begin our discussion with a general consideration of "postulational methods," for the problems which we face transcend the realm of mathematics and recur whenever a system of theorems is derived from a set of definitions and postulates. We choose as our illustration a "calculus" of symbolic logic.

Following the terminology of *Principia Mathematica,* we regard any idea which remains undefined within the system itself as a *primitive idea.* This does not mean that the idea cannot be defined at all; it merely means that if it is defined, its definition cannot be given in the terminology or the symbolism of the deductive system for the construction of which it is indispensable. Primitive ideas must be given in ordinary language and are employed only in order to clarify the meaning of the symbols which will be used throughout the system.

We shall make use of three primitive ideas, namely:

1. We shall *assume* without further definition the meaning of the word 'proposition.' In the system here used for illustrative purposes, a proposition will be represented by the letters p, q, r, s, etc.

2. We shall *assume* likewise the meaning of the term 'logical relation' and, specifically, the meaning of 'alternation.' This relation will

be symbolized by 'v,' so that 'p v q' means "*at least one* of the propositions p and q is true and both *may* be true."

3. We shall *assume*, lastly, the meaning of 'negation,' which will be symbolized by '$\sim$.' '$\sim$p' is to be read either as "not-p is true" or as "p is false."

Employing these primitive ideas, we now define the remaining logical relations, thus:

1. *Implication*, '$\supset$': $(p \supset q) = (\sim p \vee q)$ Df.
2. *Conjunction*, '$\cdot$': $(p \cdot q) = \sim(\sim p \vee \sim q)$ Df.
3. *Equivalence*, '$\equiv$': $(p \equiv q) = [(p \supset q) \cdot (q \supset p)]$ Df.

Utilizing these definitions, we now state the following five postulates:

I. $(p \vee p) \supset p$ [Taut.]
The Principle of Tautology.

II. $q \supset (p \vee q)$ [Add.]
The Principle of Addition.

III. $(p \vee q) \supset (q \vee p)$ [Perm.]
The Principle of Permutation.

IV. $[p \vee (q \vee r)] \supset [q \vee (p \vee r)]$ [Assoc.]
The Associative Principle of Alternation.

V. $(q \supset r) \supset [(p \vee q) \supset (p \vee r)]$ [Sum.]
The Principle of Summation.

From these definitions and postulates a system of "logical theorems" can be deduced.

Proof of a theorem consists in showing that it is nothing more than a specific instance of some definition or postulate which we have accepted as necessary for our system. For example, since $(p \vee p) \supset p$ is true for *any* proposition p, it must be true for the special situation in which $\sim$p replaces p, or in which $(p \supset q)$ replaces p. Hence, if $(p \vee p) \supset p$, being a postulate of our system, is accepted as true, then $(\sim p \vee \sim p) \supset \sim p$ and $[(p \supset q) \vee (p \supset q)] \supset (p \supset q)$, being theorems of the very same type of structure, must also be accepted as true. By making the appropriate substitutions they can be shown to be only specific instances of the postulate $(p \vee p) \supset p$.

From the example just given it is also clear that the procedure involved in showing that a theorem is a special instance of one of the premises of our system is that of *substitution*. This procedure is well established in the field of mathematics and offers therefore no departure from common practice in formal science. It interests us here merely because it is an element or rule which we must accept in addition to the definitions and postulates previously given.

After a theorem has been established through the procedure of substitution, it in turn may become a premise which yields still other theorems if appropriate substitutions are carried out.

Lastly, we must come to an agreement as to the form in which a proof is to be stated. We here assume the following "convention": All substitutions of values for variables will be indicated by writing the substituted proposition in front of the proposition for which it has been substituted, connecting them by the word 'for'—thus, q for p. This "index of substitution" will be followed at once by a reference to the definition, postulate, or demonstrated theorem within which the substitution is made—thus, q for p in Taut.

Care must be taken to make the indicated substitution for *all* appropriate variables in the original. If the substitution is not complete, the derived proposition is not a specific instance of the general proposition from which it is "derived," and its "proof" has no cogency.

By way of illustration of the method, we now "deduce" three principles of logic from the set of definitions and postulates given above.

1. $(p \supset \sim p) \supset \sim p$. The principle of *reductio ad absurdum*.

Demonstration:

$\sim$p for p in Taut	: $(\sim p \vee \sim p) \supset \sim p$	(1)
By Df. 1	: $(\sim p \vee \sim p) \equiv (p \supset \sim p)$	(2)
From (1) and (2)	: $(p \supset \sim p) \supset \sim p$	

2. $(p \supset \sim q) \supset (q \supset \sim p)$ Hypothetical syllogism, denying the consequent when the consequent is negative.

Demonstration:

$\sim$p for p, $\sim$q for q in Perm	: $(\sim p \vee \sim q) \supset (\sim q \vee \sim p)$	(1)
By Df. 1	: $(\sim p \vee \sim q) \equiv (p \supset \sim q)$	(2)
By Df. 1	: $(\sim q \vee \sim p) \equiv (q \supset \sim p)$	(3)
From (1), (2), and (3)	: $(p \supset \sim q) \supset (q \supset \sim p)$	

3. $(p \supset q) \supset (\sim q \supset \sim p)$ Hypothetical syllogism, denying the consequent when the consequent is affirmative.

Demonstration:

$\sim$p for p in Perm.	: $(\sim p \vee q) \supset (q \supset \sim p)$	(1)
By Df. 1	: $(\sim p \vee q) \equiv (p \supset q)$	(2)
By Df. 1	: $(q \vee \sim p) \equiv (\sim q \supset \sim p)$	(3)
From (1), (2), and (3)	: $(p \supset q) \supset (\sim q \supset \sim p)$	

These examples suffice to illustrate the general procedure of de-

riving theorems from the set of definitions and postulates.[2] As we examine the system even in this fragmentary form, we discover that it involves (i) "primitive ideas," (ii) systemic "definitions," (iii) "postulates" which, as assumed premises, determine the "content" of the system, and (iv) "rules of operation" which govern the construction of the system and which determine all transformations within the system.

Although the distinction between "postulates" and "rules of operation" may not be absolute, it is sufficiently definite to be significant; for the "postulates" are actually integral parts of the system, whereas the "rules of operation" are not. The former are "constitutive elements" of the system; the latter are only "regulative principles" guiding the process of construction.

We also know from an analysis of various deductive systems that a proposition which functions as a "postulate" or an assumed premise in one system may be a "theorem" or a derivative proposition within some other system. The "priority" of the postulates, in other words, is only a logical *prius*, not an absolute antecedence. It is a "priority" which the postulates attain through their being deliberately stipulated as true, and not one which they possess because of some inherent quality or condition.

It is true, nevertheless, that not every proposition is suitable as a premise of a logically articulate system—just as not every proposition can be an integral part of a deductive system. Premises must be universal in scope—universal at least for the system of which they are the premises; and they require no verification. In order to fulfill their purpose as premises they need be neither "descriptions of facts" nor "inductive generalizations," but they must be intelligible, independent of one another, and mutually non-contradictory (cf. Kattsoff).

Consider, for example, the basic premise of ethical rationalism: "All rational acts are good"—a premise which may be employed in the construction of a whole system of values. If this proposition is taken to be a factual generalization, then it presupposes that we know in some way what "good acts" are, and that we recognize them whenever we encounter them. If the criterion by which we know these "good acts" *as good* is their "being rational," then the generalization, "All rational acts are good," is obviously question-begging. But if the criterion is something else, then the rationalistic premise has succeeded

[2] For a fuller but elementary discussion see my book, *Critical Thinking*, Part III. For a complete system, see *Principia Mathematica*.

only in correlating that criterion with the criterion of rationality and has validated neither. The first premise of ethical rationalism, therefore, must be accepted without being verified—as must every other first premise of every other deductive system.

What is true of first premises or "postulates" is true also of all "regulative principles" which determine the actual construction of a system. No wonder, therefore, that men, in their quest for a secure basis of knowledge, have at all times tried to discover a priori propositions (in the Kantian sense) and have attempted to derive their knowledge from premises which are at once universal, synthetic, and necessarily true.

The advocates of apriorism advance several arguments in defense of their position. In the first place, they maintain, certain statements are obviously self-evident truths. For instance, the propositions, "There are propositions," "This is an affirmative proposition," "At least one conjunctive proposition exists and this is it," are all said to be self-evident truths because they themselves are what they affirm. Upon analysis, however, all of these propositions turn out to be analytical and are therefore not a priori in the Kantian sense.

The situation is clear in the case of the second and the third examples; for in each case the term 'this' requires conceptual transcription and, if conceptually transcribed, it means either "this affirmative proposition here now present," or "this conjunctive proposition here now present." But the first example—and every proposition like it—also reduces to an analytical proposition, for its truth holds only for "this" proposition.

Analytical propositions, however, contribute nothing to our knowledge, and the type of propositions here given as examples can hardly serve as premises or as regulative principles of logically articulate systems.

The apriorists may try another line of approach. They may argue that a priori premises or principles are truths presupposed in their own attempted denial. Thus, the statement, "There are propositions," may be regarded as an a priori truth because its denial, "There are no propositions," is itself a proposition. The denial of the a priori statement, in other words, is self-refuting.

Strictly speaking, however, no *self*-refutation is involved. The proposition, "There are no propositions," is not in itself contradictory. It is refuted not by the incompatibility of its terms but by a factual situation external to itself; just as the statement, "I do not exist," is

refuted only if I assert it of myself or if you assert it of yourself. As detached propositional meaning, it is free from contradiction. In every case the refutation really arises from an assertion of *two* propositions: "There are no propositions, but this is itself a proposition," and "I do not exist, but I am the one who asserts this of myself." That such dual statements refute the original proposition is granted, but this refutation is "systemic," i.e., it occurs within the context of propositions, and is not a *self*-refutation of the original proposition as such.

A third attempt to justify belief in a priori premises and principles is Kant's contention that the possibility of experience itself presupposes certain a priori truths. Inasmuch as "experience" is a matter of specific contents it cannot properly be said to presuppose anything, and inasmuch as it is articulate and specifically integrated its formal presuppositions are relative. The error of Kant was not his contention that articulate experience presupposes certain formal elements, but his assumption that these presuppositions are absolute, universal, and necessarily true. Earlier chapters of this book supply enough evidence against the rigoristic apriorism of Kant's transcendental philosophy to warrant its rejection; and the chapters of Part IV will supply more.

If the "postulates" of a deductive system are not a priori propositions in the Kantian sense, on what grounds are they to be selected? What qualifies them as premises from which the theorems are to be derived? To these questions the "formalists" reply that all postulates are but arbitrary stipulations, that there is no compelling reason why one set of postulates should be preferred to another. The development of non-Euclidean geometries and multi-valued calculi seems to show that any set of postulates yields a deductive system and that, from the point of view of formal reasoning, the most diversified systems are of equal significance, provided, of course, that each system is consistent within itself. The requirement of consistency, however, limits the selection of postulates and thus imposes a restriction upon arbitrary choice (cf. Kattsoff).

In so far as deductive systems are employed in the natural sciences, at least one additional matter must be taken into consideration. Deductive systems are useless in the natural sciences if they do not contribute to our understanding of things and events; and they do not contribute to this understanding if they cannot be applied to the phenomena under investigation. Purely formal or abstract systems may be constructed ad infinitum with complete disregard for possible

applications; but when a system of theorems is intended as an explanation of facts, a concern for empirical conditions (and not only for logical consistency) must guide the selection of postulates.

The postulates may then be generalizations which integrate established facts but which are now employed as the premises for more specialized laws, or they may be abstract stipulations designed to lead to specific laws which are descriptions of the phenomena in question. Classical mechanics, proceeding from Newton's three laws of motion, and quantum mechanics, depending upon the mathematical method of expanding a function in terms of a set of other functions, illustrate what is meant. In neither case is the resultant system totally independent of empirical data; but in neither case does a consideration of the facts affect the logical nature of the system or the rigor with which the "theorems" are derivable from the "postulates."

If the deductive system is constructed as an aid in the interpretation of empirical facts, it is, of course, desirable that it be broad enough in scope to include all relevant facts. And if the deductive system is constructed for its own sake, it is desirable that it be constructed in such a manner that all problems which arise within the system can be solved through the "rules of operation" and the postulates and theorems which constitute the system. That is to say, regardless of whether the deductive system is developed for its own sake or for purposes of interpreting phenomena, there is reason to select postulates and rules which will assure the broadest possible scope and internal completion of the system. And if this is so, then a third criterion for the selection of postulates has been found. Since the postulates themselves define the types of objects which satisfy them, they must be so chosen as to include in their range of applicability the complete universe of relevant objects. All theorems necessary for the solution of problems which fall within the legitimate range of the system must be derivable from the postulates in question (cf. Kattsoff).

Moreover, postulates which define universes of discourse cannot be meaningless; i.e., they cannot be such as to permit universal interpretation. To put it differently, since each set of postulates defines a universe of discourse, i.e., since it constitutes a selection from alternatives, each set permits only certain "interpretations" and precludes others. No set can be found that is interpretable in every way imaginable. Even logic and mathematics impose restrictions upon possible meaningful interpretations of their respective calculi. It has already been shown that if the variables of an algebraic equation were inter-

preted as standing for chemical substances, the basic laws of arithmetic might not hold; and it can be shown, similarly, that the laws which hold when propositions are involved need not necessarily hold when they are applied to physical processes.

Finally, the postulates must be so chosen that they are independent of one another. If a "postulate" is not independent of all other postulates of the same set, it is derivable from them and is, therefore, a theorem rather than a postulate (cf. Young).

The theorems must be not only consistent with the postulates but derivable from them. The set of postulates, therefore, determines which theorems are integral parts of the system and which are not. And, by the same token, the set of postulates determines the truth-value of the theorems; or it does so at least to such an extent that no theorem can have a truth-value which is greater than that of the postulates from which it has been derived.

If we disregard the problem of applicability of a deductive system, we find the minimum requirement of a set of postulates to be this: (1) the postulates cannot be meaningless; (2) they must be consistent with one another; (3) they must constitute a complete set; and (4) they must be independent of one another.

Some of these requirements constitute no problem for us. It is, for example, perfectly possible to determine whether or not a given postulate is meaningless by examining the postulate in question. No serious difficulties arise even if presumed postulates turn out to be derivable from other members of the set and are therefore, strictly speaking, not postulates at all but theorems of the system. As a matter of fact, it may often simplify actual operations within a system if key theorems which require "roundabout" proofs are included in the initial set of postulates. So long as a proposition is an integral part of the system, it makes little difference in useful application whether that proposition is a theorem or a postulate.

The situation is different, however, with the other two requirements. If a set of postulates is inconsistent, the deductive "system" derived from it is self-contradictory and thus falls asunder. Inherent contradictions make the system as a whole logically untenable. But the proof of consistency of a deductive system is, as we shall see in the next section, a problem incapable of facile solution.

And even if a set of postulates and the system which it implies have been shown to be consistent, it has not yet been proved that the set is complete. That is to say, it has not yet been demonstrated that

every problem arising within the scope of the system either has a solution or is demonstrably impossible to solve. This, too, is a matter of great significance; for, as we shall see in a later section, it involves the whole problematic of mathematical demonstration.

The Problem of Consistency

A set of postulates is in itself consistent, if it is impossible to deduce from it theorems which contradict one another; and a set of postulates has been proved to be consistent, if proof has been furnished that the deduction of contradictory theorems is impossible. The question is, Can such proof be given?

We must note that the proof in question is an "impossibility proof," and we must guard against the logical pitfalls of proofs of this type. That the required proof is involved and difficulty may be seen from the fact that even the two formidable volumes (each having close to 500 pages) of Hilbert and Bernays's *Grundlagen der Mathematik*, which are almost exclusively devoted to this proof, do not provide a satisfactory answer to all questions.

Hilbert meticulously distinguished between "mathematical" and "meta-mathematical" proofs. Whereas the former deal with demonstrations within the field of mathematics—such as the demonstration of the Pythagorean theorem—the latter are concerned with questions of validity of mathematical demonstrations. In other words, while "mathematical" proofs deal with numbers or with objects reducible to numbers, "meta-mathematical" proofs deal with postulates, theorems, and methods of demonstration which make mathematics what it is. The proof of consistency belongs to the sphere of "meta-mathematics" and is not a part of mathematics proper. Actually, the proof is needed for every possible deductive system, be it mathematical or otherwise; but the need for it has been felt especially in some fields of mathematics—such as geometry and the theory of sets—where the development of non-Euclidean systems and the deductions of paradoxes from generally accepted premises required special attention. Cayley and Klein's work in geometry, and Russell's theory of "types" as well as Zermelo's restrictive postulates dealing with the paradoxes of sets, eliminated some of the difficulties; but no proof was forthcoming that other contradictions or suspicions of contradiction cannot arise as mathematics expands. Practical solutions have been found for issues at hand, but a solution in principle and for all cases has not been found. Cayley and

Klein's work shows merely that non-Euclidean geometries are as consistent as is the Euclidean form; but it does not show that Euclidean geometry is consistent. And Russell and Zermelo exclude certain conditions which are known to give rise to contradictions of a specific type; but they furnish no proof that a deductive system as such is free from all possible contradictions.

At an early stage in the discussions (1928), it seemed that the Hilbert-Ackermann demonstrations might yield the desired result. These demonstrations consisted of two parts: (1) a strict and formalistic statement of the problem, and (2) the development of methods for a strict and formalistic solution of this problem.

The first part was achieved through the utilization of the sign of equality, '=,' and the sign of inequality, '≠.' A postulational system is free from contradiction if it can be shown that the statements $a=b$ and $a \neq b$ cannot both be derived from the same set of postulates.

The second part, it was hoped, might be achieved through a demonstration that the sequence of natural numbers is self-consistent, i.e., through a demonstration that if a given number is equal to itself, then it is impossible that this same number is not equal to itself: If, for example, $3=3$, then $3 \neq 3$ is impossible, or if $0=0$, then $0 \neq 0$ is impossible.

It is a requirement of the proof that it be possible in a finite number of steps. One of the difficulties, therefore, which can be anticipated stems from the fact that the sequence of natural numbers is infinite and that, for this reason, recourse to the principle of mathematical induction is unavoidable. Another difficulty to be expected may be stated in the form of a question: Is the deductive system self-consistent if we can prove neither that $a \neq b$ is derivable from the set of postulates, nor that it is not derivable from the set? This is the typical difficulty of all "impossibility" proofs.

Hilbert and Bernays assume as valid the general "scheme of deduction":[3] $[p \, . \, (p \rightarrow q)] \rightarrow q$, and the principle of "demonstration by substitution" as commonly employed in mathematics and in symbolic logic. But they realize also that mathematics cannot be deduced from purely logical postulates or theorems, and they accept therefore the following stipulations as the postulational premises of mathematics proper:

[3] As far as possible we shall adapt Hilbert's symbolism to more conventional American usage. The arrow '→' here means "logical" rather than "material" or "strict" implication.

A. Definitions and Conventions:

1. '0' is a number.
2. The mark (′) to the right of a number signifies "immediate successor of"; a' is thus the immediate successor of a.
3. If a is a number, then a' is a number.
4. '$\rightarrow$' signifies "logical" (as distinguished from "material") implication.
5. '$=$' signifies numerical equality.
6. '$\neq$' signifies difference.
7. '$<$' signifies "less than."
8. '$\sim$' signifies negation.

B. Postulates:

I. $a=a$
II. $(a=b)\rightarrow[\psi(a)\rightarrow\psi(b)]$
III. $\sim(a<a)$
IV. $[(a<b)\cdot(b<c)]\rightarrow(a<c)$
V. $a<a'$
VI. $a'\neq 0$
VII. $(a'=b')\rightarrow(a=b)$

This set of conventions and postulates provides a basis for a deductive system S which is essentially identical with the sequence of all natural numbers. If S can be shown to be self-consistent, then any system S' which can be shown to be as consistent as is S must also be self-consistent, and at least part of our problem has been solved. The question is, Is S self-consistent? That is to say, is every numerical equation which is deducible in S a *true* equation?

Hilbert and Bernays stipulate that an equation of S is true if and only if a replacement of its variables by "variableless" terms yields a true equation; and they can show, by simple demonstration, that for the range of the number system required for analytical geometry this condition is fulfilled. In other words, they can show that within this limited range Q the number system is indeed self-consistent. And geometry, in so far as it can be reduced to this number system, is therefore likewise self-consistent.

The proof of consistency here employed (which is essentially a reduction of the formalism of S to the sensory-intuitive realm of direct inspection) does not, however, establish freedom from contradiction for the realm of arithmetic as a whole or for any domain of mathe-

matics in which demonstration depends upon recourse to the principle of mathematical induction. At once several questions arise: (1) Is it possible to formalize the basis of arithmetic? (2) Is it possible, in particular, to formalize the principle of mathematical induction? (3) Does the addition of a formalized principle of induction to a self-consistent system of arithmetic detract from the self-consistency of the system?

Hilbert and Bernays answered the first of these questions by constructing a formalized set of postulates which is adequate as the basis of arithmetic. The following postulates serve this purpose:

I. $a = a$
II. $(a = b) \rightarrow [(a = c) \rightarrow (b = c)]$
III. $a' \neq 0$
IV. $(a = b) \rightarrow (a' = b')$
V. $a + 0 = a$
VI. $a + b' = (a + b)'$
VII. $a \cdot 0 = 0$
VIII. $a \cdot b' = a \cdot b + a$

Substitution of "variableless" terms will in each case yield a true equation. The system derivable from these postulates is therefore free from contradictions within the range of its direct applicability.

Hilbert and Bernays answered the second question by providing proof that a formalized schema of mathematical induction can be derived from two additional postulates: $A(a) \rightarrow \epsilon_x A(x) \neq a'$ and $a \neq 0 \rightarrow [\delta(a)]' = a$. Long and involved analyses show, however, that upon the basis of this set of postulates the self-consistency of the number system as a whole cannot be demonstrated. Unless, therefore, the whole procedure is re-stated and put upon a new basis, only a partial answer to question No. 3 can be given. Can the required new basis be found?

Before we answer this question let us briefly restate our problem. We have seen in Section I of the present chapter that the basic laws of arithmetic are valid with respect to the operation of addition, and that the proof of their validity can be given entirely within the realm of sensory intuitions. Recourse to the principle of mathematical induction enables us to transcend the sensory-intuitive realm and to assert the validity of the laws in question for the operation of addition involving *any* of the natural numbers. We have seen, furthermore, how the operations of subtraction and division necessitate an expan-

sion of the number system, and how all of these operations are likewise grounded in the sensory-intuitive realm where we deal directly with objects, real or imagined. Nowhere do the operations in question compel us to transcend the realm of finitude. They are performable through a finite number of steps and they lead to results which are finite. However, we know that the *complete* number system transcends the realm of finitude. The domain of all real numbers, for example, is no longer denumerable; it constitutes a specific magnitude within infinity. The complete number system, therefore, does lead us beyond the realm of finitude; and if the principles of operation, which are valid so long as we are concerned exclusively with finite magnitudes, are to retain their validity beyond the realm of finitude, they must be shown to be free from contradictions and to lead to results which are free from contradictions no matter where they are applied. It is Hilbert's contention that if a proof of "self-consistency" can be given for all ordinary arithmetic operations, then we are justified in concluding that no finite reasoning will ever disprove the results obtained through these operations—even if the results lie in the realm of the infinite; for a finite disproof of results obtained through ordinary arithmetic operations would be proof that ordinary arithmetic itself is inconsistent; and this cannot be.

Now, the line of argumentation thus far followed in Hilbert's attempt to furnish a consistency proof was concerned with the problem of the self-consistency of some given deductive system rather than with the "freedom from contradiction" of the operations which generate the system. It is at this point that Goedel's procedure of "arithmetizing" meta-mathematics itself provides a new approach and that it suggests the possibility of finding the "new basis" we have been looking for.

Goedel's procedure is, of course, involved and highly technical, and we shall forego a detailed presentation of the argument.[4] It suffices to clarify the general idea of what is involved:

In Goedel's procedure, ordinary arithmetic, in its finite aspects, is accepted as a "model" for meta-mathematics. That is to say, an attempt is made to represent each expression of meta-mathematics in the strictly linear sequence of numbers. If each sign or symbol of meta-mathematics is uniquely represented by a specific number, then each meta-mathematical expression can be represented by a unique

[4] The reader is referred to Goedel's own writings and to the discussion of Goedel's procedure as given by Hilbert and Bernays.

matics in which demonstration depends upon recourse to the principle of mathematical induction. At once several questions arise: (1) Is it possible to formalize the basis of arithmetic? (2) Is it possible, in particular, to formalize the principle of mathematical induction? (3) Does the addition of a formalized principle of induction to a self-consistent system of arithmetic detract from the self-consistency of the system?

Hilbert and Bernays answered the first of these questions by constructing a formalized set of postulates which is adequate as the basis of arithmetic. The following postulates serve this purpose:

I. $a=a$
II. $(a=b)\rightarrow[(a=c)\rightarrow(b=c)]$
III. $a'\neq 0$
IV. $(a=b)\rightarrow(a'=b')$
V. $a+0=a$
VI. $a+b'=(a+b)'$
VII. $a\cdot 0=0$
VIII. $a\cdot b'=a\cdot b+a$

Substitution of "variableless" terms will in each case yield a true equation. The system derivable from these postulates is therefore free from contradictions within the range of its direct applicability.

Hilbert and Bernays answered the second question by providing proof that a formalized schema of mathematical induction can be derived from two additional postulates: $A(a)\rightarrow\epsilon_x A(x)\neq a'$ and $a\neq 0\rightarrow[\delta(a)]'=a$. Long and involved analyses show, however, that upon the basis of this set of postulates the self-consistency of the number system as a whole cannot be demonstrated. Unless, therefore, the whole procedure is re-stated and put upon a new basis, only a partial answer to question No. 3 can be given. Can the required new basis be found?

Before we answer this question let us briefly restate our problem. We have seen in Section I of the present chapter that the basic laws of arithmetic are valid with respect to the operation of addition, and that the proof of their validity can be given entirely within the realm of sensory intuitions. Recourse to the principle of mathematical induction enables us to transcend the sensory-intuitive realm and to assert the validity of the laws in question for the operation of addition involving *any* of the natural numbers. We have seen, furthermore, how the operations of subtraction and division necessitate an expan-

sion of the number system, and how all of these operations are likewise grounded in the sensory-intuitive realm where we deal directly with objects, real or imagined. Nowhere do the operations in question compel us to transcend the realm of finitude. They are performable through a finite number of steps and they lead to results which are finite. However, we know that the *complete* number system transcends the realm of finitude. The domain of all real numbers, for example, is no longer denumerable; it constitutes a specific magnitude within infinity. The complete number system, therefore, does lead us beyond the realm of finitude; and if the principles of operation, which are valid so long as we are concerned exclusively with finite magnitudes, are to retain their validity beyond the realm of finitude, they must be shown to be free from contradictions and to lead to results which are free from contradictions no matter where they are applied. It is Hilbert's contention that if a proof of "self-consistency" can be given for all ordinary arithmetic operations, then we are justified in concluding that no finite reasoning will ever disprove the results obtained through these operations—even if the results lie in the realm of the infinite; for a finite disproof of results obtained through ordinary arithmetic operations would be proof that ordinary arithmetic itself is inconsistent; and this cannot be.

Now, the line of argumentation thus far followed in Hilbert's attempt to furnish a consistency proof was concerned with the problem of the self-consistency of some given deductive system rather than with the "freedom from contradiction" of the operations which generate the system. It is at this point that Goedel's procedure of "arithmetizing" meta-mathematics itself provides a new approach and that it suggests the possibility of finding the "new basis" we have been looking for.

Goedel's procedure is, of course, involved and highly technical, and we shall forego a detailed presentation of the argument.[4] It suffices to clarify the general idea of what is involved:

In Goedel's procedure, ordinary arithmetic, in its finite aspects, is accepted as a "model" for meta-mathematics. That is to say, an attempt is made to represent each expression of meta-mathematics in the strictly linear sequence of numbers. If each sign or symbol of meta-mathematics is uniquely represented by a specific number, then each meta-mathematical expression can be represented by a unique

[4] The reader is referred to Goedel's own writings and to the discussion of Goedel's procedure as given by Hilbert and Bernays.

sequence of numbers; and if all expressions or "theorems" are arranged in a certain order, each expression as a whole can, in turn, be identified by a number; and, lastly, the specific sequence of theorems in a proof can also be specified by numbers. It is then possible to represent the structural properties and the interrelations of theorems through properties and relations of numbers. Any contradiction in the system of theorems will then appear as a contradiction in the number system, and the freedom from contradiction in the numerical expressions implies, in turn, a corresponding freedom from contradiction in the system of theorems.

It is evident, of course, that this procedure presupposes the self-consistency of the number system and requires, therefore, a special proof of consistency for this system. This special proof can be given in a way which was first described and developed by Gerhard Gentzen, and which has since been adopted by Hilbert and Bernays. Again we shall omit the technical details of this proof. It is significant, however, that this new proof presupposes both the validity of the principle of the excluded middle and the acceptance of the principle of mathematical induction. The "finitists," therefore, cannot be expected to be satisfied with it. The "formalists," on the other hand, can raise no serious objections. But even the "formalists" admit that this new proof does not remove all difficulties. However, nothing better has been offered by either side and the proof is at least sufficient to assure the self-consistency of practically the whole domain of mathematics—provided we are willing to accept the law of the excluded middle as a valid principle of inference.

The Problem of "Solvability"

The consistency of a deductive system is not the only problem at issue. We have already pointed out the need for a proof of "solvability," i.e., the need for a proof that every problem arising within the scope of a given system either has a solution within that system or is demonstrably impossible to solve.

The demand for a proof of "solvability" is not new. Kronecker, I believe, was the first to argue that every mathematical definition should be accompanied by a procedure which enables us to show whether or not this definition is applicable in a given case. More recently, Brouwer and Weyl have raised the same issue by demanding that only such "entities" be admitted to the domain of mathematics as

can actually be constructed through a finite number of steps. But let us consider the problem stripped of its historical setting.

Let 'Q' be some property belonging to "natural numbers." To say that "the natural number n has the property Q" may then mean, for example, that "the number 2^n+1 is a prime number"; or it may mean that "in the continued decimal fraction of π the n^{th} place to the right of the decimal point is a 5"; or it may mean that "for the number $n+2$ there exists at least one Fermat number triplet (x, y, z) such that $x^{n+2}+y^{n+2}=z^{n+2}$." The question is, Do numbers possessing the property Q actually exist?

It may be the case that we can actually produce a number n possessing Q (as in the first example), and that we can thus give an affirmative answer to the question. Or it may be the case that we can show from the general character of natural numbers that no number n can possibly possess the property Q, i.e., we can show that the nature of n implies non-Q; in which case the question is answered in the negative. But if neither of these alternatives is possible, i.e., if we cannot produce a number n possessing Q, *and* if we are not able to show that n implies non-Q—as is the case with respect to the third example given above—then the "formalists" in mathematics argue that we can legitimately maintain that either there exists a number n possessing Q or there exists no such number. They argue, in other words, that the answer to our original question is either affirmative or it is negative, and that there is no third alternative. The "intuitionists," on the other hand, maintain that in all such cases a third alternative is possible and that we are not justified in holding that such a number either exists or does not exist. So long as no number n possessing Q has actually been produced, *and* so long as we cannot demonstrate that the nature of n implies non-Q, the question concerning the existence or non-existence of a number n possessing Q is undecided and must be left open.

Now, only an examination of *all* natural numbers can lead to an affirmative answer to the existential question. But since the sequence of natural numbers is infinite, no such examination can ever be completed. It is therefore "meaningless," according to the "intuitionists," to speak about a "result" of such an examination. They maintain that unless at least one number possessing Q has actually been produced, existential statements of the type "there exists a number n possessing Q" are not even real propositions. According to the "intuitionists," therefore, the principle of the excluded middle (*tertium non datur*),

which is valuable and valid in the realm of finitude, loses all meaning in the realm of infinities. And this entails, according to Brouwer and Weyl, that not all problems arising within the domain of mathematics can be solved within that domain. The question of "solvability" has thus been answered in the negative. That is to say, in addition (i) to a positive solution of a given problem and in addition (ii) to a proof of the impossibility of a solution, we must now add as a third possibility (iii) the impossibility of deciding in favor of (i) or in favor of (ii).

If the attack of Brouwer and the "finitists" succeeds, then much of present-day mathematics must be abandoned as logically untenable. All transfinite operations, the generalized theory of functions, and even the idea of limits would be affected, and only limited ranges of elementary mathematics would remain untouched. The success of Brouwer's attack, in other words, entails a disastrous revolution in practically the whole field of mathematics. But the issue is even broader and reaches beyond the domain of mathematics. Let me attempt to state it in this broadest aspect.

In the realm of finite classes of objects we find this to be true: either all members of a finite class possess the property Q, or there exists one member of that class which does not possess Q. As long as we deal with finite classes, we can verify this assertion through an actual examination of all the members of the given class. We can verify, in other words, the exhaustiveness of the stated alternatives, i.e., we can verify the validity of the *tertium non datur*. And on the basis of such verification we can justify also the following strict equivalences:

$$\sim(x)Q(x) \equiv (\exists x) \sim Q(x)$$
$$\sim(\exists x)Q(x) \equiv (x) \sim Q(x),$$

where '$Q(x)$' is a statement involving a variable, x, and a property, Q, and where '(x)' means "for all x's," '$(\exists x)$' means "there exists an x such that," and '$\sim$' is the sign of negation. That is to say, we are dealing here with propositions of general logic. In the field of mathematics, these equivalences are, as a rule, accepted as valid also in such cases where classes with an infinite number of members (infinite classes) are involved; i.e., they are accepted as valid where we step from the realm of finitude into the realm of transfinite operations.

It is obvious that we are in danger of logical confusion if this transition from the realm of finitude to the realm of infinities is made uncritically and without proper safeguards; for, as applied to infinite classes, the negation of $(x)Q(x)$ and the negation of $(\exists x)Q(x)$ have,

generally speaking, no precise meaning. Only occasionally is this not so; for occasionally an inspection of the members of an infinite class leads us, by sheer luck, to the discovery of an instance for which $Q(x)$ is not true, or, in some cases, it is possible, luckily, to deduce contradictory statements from $(x)Q(x)$ or from $(\exists x)Q(x)$. But, in the case of infinite classes, the mere fact that $Q(x)$ is not true for all x does not in itself imply that there actually exists an x which possesses the property non-Q.

The issue, more specifically stated is this: can we develop within the realm of finitude the logical apparatus which will retain full validity even in the transfinite realm so that it is possible, in principle, to assert the "solvability" of all problems which arise within a given system?

Hilbert hoped to achieve this goal through his recourse to a special "transfinite postulate": $Q(\tau x) \rightarrow Q(x)$: "If an individual τx possesses the property Q, then all x possess Q." If, for example, 'Q' means "corruptible," then 'τx' may designate a man of such sterling character that if he should turn out to be corruptible, then all men must be regarded as corruptible (cf. Hilbert and Bernays).

This "transfinite postulate" Hilbert regards as the *Urquell* or primordial source of all transfinite concepts, principles, and theorems, for, if the symbols for "all" and "existence" are added, it is possible to derive as theorems all purely logical *and* transfinite principles:

$(x)Q(x) \rightarrow Qx$ (Aristotle's principle)
$Q(x) \rightarrow (\exists x)Q(x)$ (Principle of existence)
$\sim (x)Q(x) \rightarrow (\exists x) \sim Qx$
$(\exists x) \sim Qx \rightarrow \sim (x)Qx$
$\sim (\exists x)Qx \rightarrow (x) \sim Qx$
$(x) \sim Qx \rightarrow \sim (\exists x)Qx$

Through the employment of the last four of these theorems the statements of equivalence which are valid for finite classes, and the principle of the excluded middle are acknowledged as valid for infinite classes. It is therefore evident that the problem of "solvability" will have been solved if it can be shown that the introduction of the "transfinite postulate" entails no contradiction for the system as a whole. The proof of "solvability" thus resolves itself into a proof of consistency; and everything we have said concerning the latter is therefore relevant to our discussion of the former. It is, however, superfluous to repeat here what has been presented at such length in the

preceding section. The conclusion there stated is also the conclusion to which we must come here: Operations with transfinite magnitudes are permissible and valid *if we accept the principle of the excluded middle.* The "finitists" refuse to rely upon this principle, and if they carry their point, a major part of contemporary mathematics must be abandoned as logically unsound.

In view of what has been said in preceding chapters about the basis of the *tertium non datur* in first-person experience, about its interdependence with the principles of identity and contradiction, and about its employment in the construction of formal calculi, it is obvious what our choice will be in this matter. The abandonment of the principle of the excluded middle entails not only too great a sacrifice in the field of mathematics, it leads also to insurmountable difficulties in the realm of semantics and of rational thought.

Mathematics and the Physical World

One additional problem must be considered before we bring this chapter to a close; it is this: Since mathematics is in all essentials a deductive system the cogency of which depends upon definitions, postulates, and logically rigorous methods of proof, i.e., since mathematics is a system of ideal constructs, how is it possible that its theorems are valid for the real things of a real world? What, in other words, justifies the application of pure mathematics to the physical realities of the world about us?

To this question different answers have been given at different times. The Pythagoreans, for example, maintained that numbers are the very essence of things, and that mathematics is applicable to the real world because that world is ultimately itself number and a harmony of numbers. Kepler assumed a fundamental harmony between a mathematical structure of the cosmos and our faculty of understanding "which seems to be such from the law of creation that nothing can be known completely except quantities or by quantities." Kant, on the other hand, argued that "the space of the geometer is exactly the form of sensuous intuition which we find *a priori* in us, and contains the ground of the possibility of all external appearances," and that therefore "the latter must necessarily and most rigidly agree with the propositions of the geometer, which he draws not from any fictitious concept, but from the subjective basis of all external phenomena, which is sensibility itself" (*Prolegomena*).

Galileo also faced the problem and solved it in his own way. In his *Dialogues Concerning the Two Great Systems of the World*, one of

the interlocutors, Simplicio, admits that mathematical demonstrations may be cogent and logically beyond reproach so long as we remain in the domain of pure mathematics, but maintains that such demonstrations are applicable to nothing in the physical world. In theory it may be true, for example, that a sphere touches a plane in only one point; but in the world of physically real spheres and planes it is manifestly not so.

To this challenge Galileo replies that the discrepancy between mathematical theory and physical fact is the fault neither of geometry nor of physics, but is due entirely to the inability of the investigator to calculate correctly; for the investigator, Galileo argues, begins by defining a generalized "ideal case" of some particular process of nature under observation; then he introduces such qualifying propositions as will make the "ideal case" fit the factual situation; and only to the extent to which he succeeds in doing this will there be harmony between his theory and the real world. It remains true, therefore, that in theory sphere and plane touch each other in only one point; but in the world of physical things we must allow for a certain "roughness" of surface and must take into consideration the "pressure" of a physical sphere upon a physically real plane. That is to say, the "ideal case" must be appropriately modified to fit the facts of the physical world.

Let us consider another example. Galileo's law of falling bodies, $s=\frac{1}{2}at^2$, is, strictly speaking, valid only for "ideal cases," i.e., for cases of bodies falling in a vacuum. When the law is applied to bodies falling under ordinary conditions in the actual world (or to balls rolling down an incline), allowance must be made for air resistance and other disturbing factors. The law, in other words, is strictly true only in theory, and the harmony between the law and the facts which it interprets can be preserved only through a judicial adjustment of the data in question.

The implication of all this is that the relation between mathematics and physical phenomena is not an a priori harmony but the result of design and construction. Objects, of course, can be *counted* because each object can be regarded as unity, and because counting is essentially a sequence of adding unities. But when we *measure* physical things or intensities, the result is at best but an approximation to the actual magnitude (depending on the sensitivity of our measuring device and on the skill of the operator), and the exactness of a mathematical statement or equation is an idealization of the empirical facts. The harmony between mathematical demonstration and actual

data is established and preserved only through appropriate interpolations.

From the point of view here advocated we can dispense with Kant's apriorism and can yet account for the applicability of the theorems of geometry to the world of things; for Euclidean geometry is essentially the geometry of rigid bodies and is perfectly valid only in a world in which rigid bodies exist. Euclidean geometry, therefore, is an "ideal case," and only to the extent to which real bodies of the physical world approach complete "rigidity" can Euclidean geometry be regarded as valid for that world. In a world of gravitational fields, i.e., in a world such as ours, no bodies are absolutely rigid. Euclidean geometry, therefore, is but a first approximation to the conditions and spatial relations of that world. If we accept the ray of light as the "material representation" of a straight line, we shall find that a modified Riemannian type of non-Euclidean geometry gives us a better description of the geometrical relations in the world of things than does Euclidean geometry. But whether we regard "physical space" as Euclidean or as non-Euclidean depends no longer on a pre-established harmony between mathematics and physical reality, nor on an apriorism of the Kantian type. It is entirely a matter of appropriate interpolations and of regarding some basic geometrical concept as exemplified by a specific physical thing (rigid body or ray of light, respectively). Pure geometry provides the "ideal cases" which we apply to the facts through appropriate interpolations and constructions, and the geometry of physical space is actually no longer a branch of pure mathematics but a branch of physics; i.e., it is a theory concerning the interrelations of physical things. Its truth or falsity, therefore, can no longer be decided on a priori or purely deductive grounds but only on the basis of empirical evidence. And thus we have left the realm of pure mathematics and have entered the borderland of the empirical or natural sciences.

In an earlier section we have shown how and why functional equations may serve as laws governing all those physical phenomena which can be graphically represented by certain lines in a system of coordinates. Acceptance of a Euclidean or non-Euclidean geometry is ultimately but a choice of a system of coordinates—of that system of coordinates which will yield the most adequate equations (laws) for the description of "curves" representing physical events. The applicability of mathematics to the processes of nature thus finds a solution which is neither mysterious or mystifying, and which requires no metaphysical adumbration.

PART IV

EMPIRICAL KNOWLEDGE

CHAPTER VII

SCIENTIFIC METHOD

Formal knowledge, as discussed in the two preceding chapters, constitutes at best only one aspect of scientific knowledge; for the sciences, as we ordinarily understand them, deal with the "empirical facts" of experience rather than with purely logical relations or imaginary conditions. They depend upon "observation" and "experimentation," and upon the construction and verification of hypotheses. They have developed a method of investigation all their own and may best be characterized by a reference to this method. One question, however, must first be answered: What is the aim or purpose of empirical science? What is the ultimate goal to be achieved in the natural and social sciences?

The Aim or Purpose of Science

It has been stated at various times that the aim of science is (1) to give an adequate and complete *description* of reality, and (2) to *explain* the processes and phenomena of nature; and the language employed has often suggested that no sharp distinction was made between description and explanation. This loose use of terms, however, is confusing rather than helpful and does not contribute to our understanding of the real aim of science. On the other hand, if we define our terms carefully, the distinctions which such definitions imply may at once cast new light upon the problem which we face here.

By 'description' we shall here mean a simple enumerative account of the observable features or qualities of individual facts or events. Thus when an archeologist gives us an account of the distinctive features of the ruins of some ancient building which he has discovered, he gives us a description of what he has found. And, similarly, when a geographer gives us an account of the natural contours of a landscape, i.e., when he tells us that there are such and such mountains located in such and such a part of the region, that such and such rivers flow in such and such directions through the valleys, and that the plains are inhabited by such and such people in such and such communities, he is giving us a description of the landscape in question. Descriptions,

in other words, are but answers to the question, What are the particular and observable facts?

Description, as here understood, is a first step in any science; and we encounter it in physics and astronomy no less than in biology and the social sciences. But in no case is a description as such already an explanation.

It is obvious that various degrees of accuracy can be obtained in description; and the scientists will always aim at the most accurate account of the observable facts. Since the most accurate descriptions are those which involve quantitative statements based upon measurements of various kinds, the scientist will always aim at precise quantitative descriptions. Hence, when an astronomer describes a solar eclipse, he will note not only the general features of that event, but will take care to determine to within a second the exact duration, and to within a fraction of a degree the spatial extent of the obscuration. All references to "orbital motions" and to the "constellation of stellar bodies," however, transcend mere description and give us a first glimpse of an explanation.

An *explanation*, therefore, as here intended, is any attempt to *account for* the described facts or phenomena.

The obscuring of the sun in midday, i.e., the solar eclipse, is thus accounted for or explained when it has been shown that *because of* the specific structure of the solar system (which includes sun, earth, and moon) and *because of* the nature of planetary and lunar motions, sun, moon, and earth can get into such a position relative to one another that the moon cuts off the rays of light from the sun which otherwise would have reached the earth. The explanation here involves relating the phenomenon in question (the eclipse) to accepted facts pertaining to the structure and the motions of the solar system. Somewhat loosely speaking we may say that 'to explain' a phenomenon means here to make evident its causes, i.e., the conditions which "bring it about." But let us be more precise in this matter.

In the case before us, the conditions which "bring about" the phenomenon are the structure and the motions of the solar system. Such a general reference as this is, however, insufficient as an explanation. We must, in addition, show *how* the structure and the motions of the system can and do "bring about" the observed result; and in doing this, we must examine both the structure and the motions more closely. We then discover that the motions of the planets and satellites in our solar system are governed by specific laws

—such as the three laws of motion first formulated by Kepler. Given the distribution of the masses of sun, earth, and moon, the constellation producing a solar eclipse can be deduced from those laws and is "explained" through this deduction. Accepting the structure of the solar system, we thus "explain" the solar eclipse by showing that it is a consequence of the laws governing the motions of some of the stellar bodies that are integral parts of the system.

That this is only a partial explanation is obvious; for it *accepts as given the structure of the system.* The explanation would be complete if it could be shown how this structure itself has come into being. Without considering in detail the various scientific cosmogonies, we can say that such an explanation involves laws which govern the "near-collision" of stars and the "tidal friction" which tore huge masses of gaseous substance out of the body of the sun, laws which govern the condensation of gases and the distribution of the cooling masses in their respective orbits.

It is true, of course, that even this is no *absolute* explanation. It does not account, for example, for the fact that there *are* stars or that the stars move in galaxies, nor does it explain the existence of galaxies or, for that matter, the existence of anything. But *absolute* explanations are not found in any science, nor are they to be expected. Even theology cannot explain everything; for if we grant for the sake of the argument that God has created the world and everything that is in it, God's own existence must be assumed and is not explained through a reference to his own act of creation.

Lest it be argued that the case of a solar eclipse provides an illustration of scientific explanation which, in its essentials, is not typical for all fields of investigation, we consider next an example of scientific explanation from the field of experimental physiology. When Claude Bernard investigated "poisoning with carbon monoxide," he observed that the blood in all the vessels of all animals killed by this gas was scarlet in color. This suggested to him that the blood of the poisoned animals was like arterial blood in containing oxygen. Upon further investigations, however, this idea was found to be false. Additional experiments revealed that the carbon monoxide expelled all oxygen from the blood and remained itself chemically so closely combined with the hemoglobin that it could not be displaced either by oxygen or by other gases. Death resulted, therefore, from a chemical reaction in the blood which drove out of the blood the oxygen essential to life. Given the nature and function of hemoglobin and the nature of carbon

monoxide, death from carbon monoxide poisoning is now fully explained as the result of a chemical reaction which, in turn, is governed by well-established laws.

From what has been said in connection with the two examples of explanation and from the general nature of scientific knowledge, it is evident that in every explanation there remains an unexplained residue —some structure or function or quantity which has to be accepted as "being so." But the point to be noted is that at least in scientific enterprise an explanation consists in showing that the phenomenon under investigation follows from the "given" or accepted "state of things" *in strict conformity with some law or laws.* When no recourse to laws is had, we either do not deal with scientific explanations or we are confronted with the first gropings after such explanations.

If this is at all a correct interpretation of the actual situation, then the nature and function of laws deserves special attention. We shall take up this problem in detail in the next chapter. Here it is necessary, however, to make certain preliminary distinctions, for they contribute materially to our understanding of the aim and purpose of science.

A description, so we have said, is a simple enumerative account of the observable features or qualities of individual facts or events. There is nothing compelling about a description save the fact that the empirical data are found to be so and not otherwise. The data, however, vary from case to case. A law, on the other hand, is universal and necessary for all relevant cases. It is not an enumerative account of the features or qualities of individual facts or events but an integrative statement of the essential attributes and relations of classes of things and events.

In the simplest cases, a law may state the functional interrelations of directly observable or measurable magnitudes. It is then essentially identical with the integrative concept as defined in an earlier chapter. Galileo's law of falling bodies and Kepler's laws of planetary motion are examples of this kind. Galileo's law, for instance, defines strictly and with utmost precision the concept 'free fall' and expresses *as universal and necessary* the ratio of the distance traversed to the square of the time of falling. Kepler's three laws, taken together, accomplish a corresponding integration for planetary motions. We shall refer to laws of this type as "laws of the first order" or "laws of the first level of integration."

It is a well-known fact that Newton's law of gravitation unites in

one equation the essential truths of Galileo's law of falling bodies and of Kepler's three laws. That which Kepler and Galileo treated as separate and distinct phenomena is now shown to be but a diversified manifestation of one and the same condition in nature. The integrative power of Newton's law thus surpasses by far the corresponding power of the restricted formulations of Galileo and Kepler. The latter are special cases of the former and cover limited areas only. Newton's law of gravitation may therefore be said to exemplify "laws of the second order" or "laws of the second level of integration."

Einstein's "principle" of relativity, linking together into one broad conception the laws of mechanics and of electrodynamics, and the "principle" of quantum mechanical resonance, combining as it does in one equation the field of physics and the field of chemistry, are examples of a still higher order of synthesis and may be regarded as "laws of the third level of integration."

The desideratum of the sciences seems to be to find a law or principle so broad in scope that it includes *all* phenomena irrespective of any particular field of investigation—a law or principle, in other words, from which all specialized laws of the lower levels of integration can be derived in a way comparable to that in which Newton's law of gravitation can be derived as a special case from Einstein's law of relativity, or in which the structural laws of organic chemistry follow from the law of resonance.

Whether or not such an "ultimate law" can ever be found is a question which need not occupy us unduly at this time. What interests us here is the double fact that, in principle, all sciences aim at the discovery of laws which will explain the observed phenomena, and that they aim at the greatest possible unification or integration of these laws.

Some sciences, obviously, have advanced much farther along the road to this goal than have others. Some sciences, indeed, are as yet in possession of very few laws and must of necessity be satisfied for the time being with descriptions. But in so far as the so-called "exact" sciences—physics and chemistry—serve as models, the trend in the sciences is as here indicated. A glance at psychology, sociology, and economics reveals this trend clearly.

If in the light of the preceding discussion the aim and purpose of science is to be defined, we can say that science, starting with adequate and concise descriptions of individual facts and events, aims at an explanation of these facts and events through recourse to universal

and necessary laws, and that it sees its ultimate aim in the unification of all laws in one explanatory system which integrates the whole of experience and makes it deducible from the basic postulates or principles of the system.

We are admittedly far from having attained this goal, but the trend, I believe, is unmistakable.

And what is the purpose of it all? Briefly stated, it is the integration and explanation of the facts of experience and the utilization and control of the forces and conditions in nature which make our human lot what it is, to the end that we may improve the chances of man's well-being and cultural progress.

Observation and Measurement

We stated at the beginning of this chapter that the empirical sciences may best be characterized by a reference to the specific method of investigation which they have developed, and that this method depends upon "observation" and "experimentation." We now return to this point, intent upon a closer inspection of both observation and experimentation.

All investigators agree that scientific method is intimately bound up with "observation"; that it starts with observation, leads to observation, and requires a constant check through observation. But what is an "observation"?

In its strictest sense the term 'observation' signifies a "taking note" of some specific contents of first-person experience, a discernment of "irreducibles" in that experience, or, if you prefer, an "inspection" of what is "given" in immediate and non-inferential experience. In an ultimate sense, all other so-called "observations" must be reducible to this; for there is no other access to the qualitative and quantitative aspects of reality save through the inspection of the dimensions of otherness in first-person experience.

Once this is granted, we may speak, however, of 'observation' also in the less precise sense of a "taking note" of *things*, of their occurrence, their distribution, and their change, and may supplement our inspection of the objects of first-person experience by a reference to "observed facts" of the external world.

But whatever meaning of the term 'observation' we accept, we must keep in mind that it never designates a purely passive attitude on the part of the 'observer.' The observing mind, from the very beginning, is actively engaged in an integration and interpretation of the contents

of experience and in making distinctions, in isolating, comparing, and relating in various ways the "data" of observation. Our discussions in preceding chapters can have left no doubt on this point.

The integrating function of mind is, of course, basic to all experience of meaning, but in observation more than this is involved, for the purpose in the mind of the observer determines to a large extent what will be observed in any given situation. Chance observations of all sorts of qualities, aspects, or phenomena are possible; but if our purpose is to determine, for example, the space-configuration of certain objects, we may overlook completely their variations in color and may not notice the differences in their textures; or if our intention is to study the color of objects, we may entirely disregard their form and size. The narrowness of the focal point of our attention tends to exclude from observation everything which is not directly connected with the purpose we have in mind.

The accuracy and the fruitfulness of our observations depend, furthermore, upon the knowledge which we possess in the field of observation. The engineer examining a complicated machine will observe a cosmos of interdependent parts where a layman sees only a "chaos" of wheels and wires and tubes, just as the football expert will observe the design of a play where the uninitiated spectator sees only a bewildering turmoil of running and tumbling young men.

The study of illusions makes us distrustful of some observations, for we realize that things are not always what they seem to be. Seeing is not necessarily believing. Our own observations must be checked by the observations of others and, if possible, instruments must be employed which supplement our senses and preserve records of what has occurred. The use of blue litmus paper, for example, provided the first *objective* criterion for the discrimination of acids, just as the use of clocks and balances made possible an objective determination of "durations" and of "quantities."

But the introduction of instruments serves another purpose. Telescopes and microscopes extend the range of our vision and disclose aspects of reality which are inaccessible to the unaided eye. They permit us to transcend the ordinary range of experience. Such transcendence is possible because it involves only an expansion of our horizon of observation and not a "transcendency" of experience itself. The new contents of experience disclosed by the instruments can be linked up in a sequence of steps with familiar contents of "unexpanded" experience. And in this sequential relation of the "expanded" to the

"unexpanded" experience, and in this relation only, lies the justification of our acceptance of the new contents as data for science.

We transcend the contents of direct experience in a different way when we resort to what has been called "observation via causation." Modern physics, in particular, abounds in observations of this kind. It is impossible, for example, to observe directly a single electron. The physicist, therefore, constructs a "cloud chamber" in which, in accordance with complicated theories, the fast moving electron leaves a track of ionized vapor droplets which can be photographed. The electron now becomes indirectly "observable" as the *cause* of a "track" on the photographic plate. Direct observations, pointer readings, and causal inferences are thus combined in a complex process which, as a whole, may also be regarded as "observation." The element of hypothesis in scientific observation is here obvious, and it is clear that "observation" in the advanced fields of science is not the simple and elementary process of "inspection" referred to above. The observational element in the strict sense, i.e., the direct reference to some sense quale, is not absent, but integration, inference, and interpretation play an important part in all such "observations."

Scientific observation, while concerned with qualitative distinctions, ultimately aims at quantitative discernments. The reason for this emphasis on quantity is that quantitative distinctions can be made much more accurate than qualitative ones; and the essence of science is accuracy. Scientific accuracy demands that we say, not merely that an object A moves faster or is heavier or is warmer than object B, but that it moves *so and so much* faster, is *so and so much* heavier or *so and so much* warmer.

Whenever quantitative distinctions are to be made, certain procedures must be invented which enable us to assign specific numbers to the various properties of the objects. The most fundamental procedure is, of course, counting. When objects can be counted, the laws of arithmetic (which find their justification in counting) can be applied. Here, then, we encounter the first and most direct linkage between mathematics and reality—a linkage which at once justifies the application of formal laws to the empirical data of experience. Objects can be counted, and counting is the basis of those laws.

The process of counting culminates in "statistical surveys," and these surveys provide observational data from which scientific interpretation may proceed. But counting and statistical surveys constitute

only one type of quantitative determination; another type involves "measurement."

Counting and statistical surveys involve an enumeration of *objects* or *events*; measurement, on the other hand, is a procedure for the assignment of quantitative values to certain *properties* of objects and events.

One of the basic measurements is that of distance or length. It is obtainable because all things which belong to the world about us are spatially extended or spatially coexisting. We may "measure" the length of an object by the span of our hand or the reach of our arm, and we may "measure" the distance between two objects by the number of steps it takes us to go from one to the other. Such "measurements," however, are not objectively dependable, for our steps may be long or short, and your arm and my arm may not have the same "reach." Nor are such crude "measurements" sufficiently accurate for distances which are so short that even the smallest deviations from the employed standard are important. The scientist, therefore, substitutes a "measuring rod" for his steps and arm reaches, and subdivides this "standard" into fractions of predetermined length. The "yardstick" and the "meter" are such standards; but the selection of the unit measure is a matter of arbitrary choice and is determined solely by considerations of convenience. The astronomer may thus accept the "light-year" as his basic unit, whereas the physicist, dealing with subatomic phenomena, may select instead the wave length of a red spectral line emitted by cadmium. Since distance or length is continuous, one standard measure can always be translated into units (or fractions) of all other standard measures—just as inches can always be translated into centimeters, and centimeters into miles.

Whatever the standard which we accept, if it *is* our standard, we must regard it as "rigid" and as unchanging; but we know now that in the world about us "rigid bodies" do not exist. Fluctuations in temperature or in gravitation, for example, "distort" our standards. We must therefore allow for changes and variations and must make the necessary corrections. But how can this be done?

We may specify, to consider a specific example, that the standard "meter" is the length of a certain platinum rod at 70° Fahrenheit. But if we do that, we make the standard measure of length dependent upon a measure of temperature, and it can readily be shown that the measure of temperature already presupposes a measure of length; for "degrees" of temperature are defined in terms of the length of a

column of mercury (or whatever may be used in our thermometer). Are we not caught in a vicious circle?

The physicist has learned to avoid this circle by a process of successive approximations. Defining a platinum rod of a certain length as his standard, he has obtained a first approximation of the true unit of his measure. With the aid of this "approximate" standard the physicist then constructs a thermometer which enables him to discover that his original standard of length is subject to changes as the temperature of its immediate environment fluctuates. A change in temperature, however, produces only a relatively small change in the length of the platinum rod. Hence, if this rod is used in constructing the scale of the thermometer, the error in the scale will be very small. If now the standard of length is re-defined relative to some specific degree of temperature, the possible error in the standard measuring rod is one of the second order of small quantities; and this means that the error is so small that for all practical purposes it can be neglected (cf. Lenzen).

Once we have obtained a measuring rod of standard length, we can use it in the construction of the geometry of "physical" space. The conception of a cube, for example, is in itself a conception belonging to pure geometry; but when we measure the sides and angles of a physical thing and find that our measurements fulfill the specifications of a cube, we can then say that the thing *is* a cube and that all the geometrical proportionalities of cubes will be found in this thing. Through the application of the measuring rod pure geometry thus becomes a "science of nature," and as a science of nature it is exceedingly useful. The proportionalities of a triangle, for instance, may now be used to measure distances inaccessible to any other measuring device. The method of "triangulation" is widely used in astronomy and in soil surveying.

"Triangulation" as a method of measuring distances involves, of course, a certain amount of "theory"; but this is not an exceptional case. On the contrary, no method of measurement is completely free from all elements of theory, and some methods depend upon theory to a surprising degree. "Measuring" astronomical distances through the use of "Cepheid variables," for instance, presupposes and involves a whole complex of observed "facts" and interpretative "theory." The "Cepheid variables" are stars whose brightness varies in rhythmic periods of rapidly increasing luminosity and a somewhat slower diminution. The Cepheids in the Magellanic Cloud are all at approximately the same distance from the earth (as determined by the "paral-

lactic" method). Studying these variables, Miss Leavitt reached the conclusion that the period of light-fluctuation depends in each case on the "candle-power" of the Cepheid. The slower the rate of fluctuation, the greater is the candle-power of the star, and the faster the rate of fluctuation, the lower is the candle-power. Cepheids which fluctuate with equal rapidity have the same "intrinsic" candle-power. Since Cepheids are exceptionally bright and since their rhythm of fluctuation is so characteristic that they cannot be confused with other stars, they can be observed at tremendous distances from our earth; and since the rate of fluctuation is an index of the candle-power of each star, the apparent brightness of the Cepheids gives us a fair measure of their distance. Since, lastly, Cepheid variables are very freely scattered throughout space, they furnish an important "measuring device" for all regions of the sky. If, for example, a Cepheid has a fluctuation period of 40 hours, it is approximately 200 times as luminous as the sun, i.e., it has a candle-power of 6.46×10^{29}. If the period of fluctuation is 10 days, the luminosity of the star is 1600 times that of the sun; i.e., the Cepheid has a candle-power of 5.17×10^{30}. Now, if a Cepheid variable having a 10-day period of fluctuation appears as a star of magnitude 16, i.e., if an observer on earth receives from that star as much light as he would from a single candle placed at a distance of 570 miles, then the difference in candle-power between 1 candle and 5.17×10^{30} candles is an index to the distance of the Cepheid. Since the apparent luminosity of a star diminishes as the inverse square of the distance, we can deduce from the figures given that the star in question is about 220,000 light-years distant—although allowance for the dimming effect of the obscuring matter in interstellar space will reduce this figure somewhat (cf. Jeans). What is important here for our problem is the large amount of "theory" that enters into such "measurements" as these. Should the theories and assumptions upon which the "Cepheid method" rests prove untenable, the whole procedure would lose its significance.

Most of our measurements of length are, of course, carried out relative to the surface of the earth, and they are measurements of stationary things. In all such cases all that is required is that we apply our measuring "rod" (whatever it may be) to the object to be measured. A new problem arises, however, when we, as stationary observers, attempt to measure the length of a body in motion; for in this case we cannot apply our measuring rod directly.

We may, of course, measure the length of a moving body indirectly by marking off its end points on a stationary frame of reference as it

passes by, and by measuring the distance thus marked off by the application of the measuring rod. But if we do this, it is necessary to mark off the end points of the moving body simultaneously, for otherwise we cannot obtain the true measure of the body's length. Simultaneity, however, as Einstein has shown, is relative, and the measured length of a moving body is, therefore, also relative; it is, as a matter of fact, inversely proportional to the velocity of the measured body.

The measurement of time is, as a rule, accomplished by means of some periodic motion—the swinging of a pendulum, the rotation of the earth, or any other rhythmic occurrence. The only requirement is that the process used as the standard or as the unit measure in the time scale be subject to constant conditions and be itself unchanging. Actually, it may not be possible to eliminate all irregularities or to control all conditions which tend to affect our "clocks." If we know the nature of the irregularities—such as the effects of variations in gravitation upon the swing of the pendulum—we can allow for them without impairing the usefulness of our measuring device.

A new problem in the measurement of time arises when we attempt to transmit *our* "local time" to *some other* system of "local time" which is in motion relative to our system. A comparison of clocks is then possible only by a sequence of light signals (or some comparable device). Assuming that the velocity of light is constant in all directions —and without this assumption (which finds experimental support in the negative result of the various "ether-drift" experiments) the comparison of time intervals in different "systems" is impossible—we send a signal at the beginning of our time unit, and another signal at the end of the time unit. Since light travels at a finite rate of speed, our signals will be received in "the other" system after the lapse of a brief interval, the length of which is determined by the equation

$$t_2 = t_1 + \frac{d}{c}$$

where t_1 is the time of emission of the signal, t_2 the time of receiving it in "the other" system, d is the distance between the two clocks, and c the velocity of our signal (light).

If "the other" system moves relative to our own, the interval given by the right side of the equation is obviously not the same for the two signals, for d is a variant. The time unit as indicated by the interval between our two signals will therefore vary for "the other" system in accordance with the motion of that system. But if time units are thus relative to the frame of reference, simultaneity must also be relative

and must be dependent upon the same conditions of motion. Einstein's special theory of relativity is thus deeply grounded in the very principles of measurement.

We consider, finally, the measurement of a basic physical property, that of weight.

If we lift a material body—such as a book or a stone—and support it with our hand, we observe that a certain "force" is required for the lifting and the supporting. I shall not attempt to define 'force' at this time but shall take the word in its common sense meaning and shall maintain that the feeling of "strain" in our muscles is an index of the "force," and that our kinesthetic sensations disclose something of its nature and magnitude. When we support the body in mid-air, the "force" of our muscles is counteracted by a downward "push" on the part of the suspended body; i.e., it is counteracted by the tendency of the body to fall to the ground. This downward "push" or "tendency" to fall is a manifestation of the "weight" of the body. Weight, in other words, is a "force" which balances other "forces."

If such is the case, then we can measure weight by means of a two-armed lever; for the laws of the lever determine how we can find the weights of various bodies in terms of some assumed "unit" or standard. For example, if the two arms of the lever are homogeneous and equal, and if the "unit" (or any multiple thereof) is attached to the free end of one arm, then any object attached to the free end of the other arm will be equal in weight to our "unit" (or to the given multiple thereof) if it "balances" the lever. If the levers are unequal in length, the weight of the object stands to the weight of our "unit" in a relation which is inversely proportional to the lengths of the respective lever arms.

Furthermore, since weight is a "force" which balances other "forces," it can be employed in the measurement of these other "forces." Weight thus enables us to measure the "tension" in a spring and the "power" of a magnet. The whole range of quantitative determination of "forces" is opened up and physics can now proceed in every direction. And if all "forces" can be measured, then the possibility of a quantitative interpretation of all phenomena of nature, of all things and events in the external world, has been assured.

Classification and Induction

The observation and measuring of given phenomena is only a first step in the acquisition of scientific knowledge. The facts obtained

through observation and measuring must be interpreted and coordinated and must be interwoven in a coherent system of interdependent propositions. A first stage in this process of "elaboration" is the classification of facts.

On the basis of observed or measured similarities, individual data may be combined into groups or classes; and on the basis of common "class properties," different classes may be combined into still larger or more comprehensive classes. In this way a certain primitive order is established in the realm of observation.

Traditional logic applies the name 'genus' to the inclusive class, and the name 'species' to all included classes. But what is "genus" in one contextual setting may be "species" in another; and what is "species" when it is considered in one perspective may be "genus" when it is viewed in another. 'Genus' and 'species,' in other words, are purely relative terms and designate no metaphysical "entities."

Traditional logic, furthermore, has formulated at least five distinct rules which should govern all classification. But of all these rules only one is really important, the rule, namely, that in any classification the different species of a given genus should be mutually exclusive. That is to say, if we arrange our facts in groups, these groups must not overlap. All other so-called "rules of classification" are adequately covered by the general stipulation that the value of a classification is determined by its success or failure in achieving the purpose which was intended.

The classification of objects is especially important in the so-called "descriptive" sciences—such as zoology and botany; but it is an elementary stage in all sciences and has its beginnings in pre-scientific experience (see Chapter III).

The more fundamental the basis of classification, the more significant is the grouping of the facts. The scientist, therefore, cannot be satisfied with some superficial similarity of objects as the basis of classification but must strive to discover the *essential* similarities. How carefully he must choose his basis of classification and how difficult it is to discover "true" species has been shown by Jepson in his classification of the flowering plants of California and, in a more general sense, by Dolzhansky. Jepson arranges the various species on the basis of the following considerations: (1) What is the life history of the separate species? (2) What is the bio-geographical or ecological status of the plants? (3) What are the specific characters which the individual plants display at the fringe of the area of greatest develop-

ment? (4) What specific plant organs are present or absent, and if present, what is their structure? (5) What variations occur in the organs of any one member of the prospective species, or in the organs of a number of such members if they have a common parentage? (6) What are the reactions of the plants in question to injuries and mutilations?

Jepson finds even then that the classification must always be adapted to some problem and must be carried out with some purpose in mind. If, for example, the members of the genus *Arctostaphylos* are classified on the basis of morphological characteristics and biometric measurements, not more than five or six species can be found in California; but if the classification is based upon the plant's reactions to chaparral fires, at least twenty-five species can be discerned, all of which give fundamentally different yet constant reactions correlated with geographic and ecologic segregation. Thus, while classification is an elementary stage in the development of scientific knowledge, it is not necessarily a simple and easy task.

Classifications of the type just considered involve an element of induction, and induction has always been regarded as the very essence of scientific method. But what, precisely, is "induction"?

Even a cursory examination of concrete instances reveals that the term 'induction' is not altogether free from ambiguities and that, as a matter of fact, "induction" may occur in several forms. There is, however, one element involved in "induction" which is common to all forms and types and which may therefore be regarded as the essential element of "induction" as such. This element is the ideational transition from individual cases or from propositions pertaining to *some* cases to universal statements or assertions pertaining to *all* cases.

At least four types of "induction" may be distinguished. I shall call them "intuitive," "summary" or "enumerative," "mathematical," and "demonstrative" induction, respectively, and shall discuss them in this order.

(1) "Intuitive" induction is encountered whenever we apprehend directly the universal in the particular. Two distinct forms of this type of induction are possible. First, it may be the case that we discern as a universal the predicate which we attribute to some specific object —as when we say, "This is a rose," or "This rose is red." To identify "this" as a *rose* or "this rose" as *red* is to think of "this" or of "this rose" as possessing qualities which can be defined only through universals. "Discerning a universal in the particular" here merely means

seeing that a particular "this" shares some attributes with other objects of experience. It is a matter of semantics rather than of logic or scientific method.

It may be the case, secondly, that we regard a proposition as a whole as universal, not merely one of its terms—as when we assert, "All whales are mammals." And it is this type of "discerning the universal in the particular" that raises a new problem—the problem, namely, of generalization, i.e., the problem of a transition from "*some* are" to "*all* are." And this problem, according to many thinkers, is the problem of induction par excellence.

Psychologically the transition is facilitated by the fact that we may observe *many* instances of the same phenomenon; and it is easy to believe that whatever is true of *many* instances of a certain type is true of *all* of them. Psychological "facility," however, does not in itself provide a logical warranty for the assertion of a universal, and the accumulation of instances as such has little bearing upon the rational certainty of the generalization.

Let us suppose, for example, that somebody strikes successively the keys C, E, and G on the piano. We then hear three sounds in an ascending order of pitch; and from this single instance of auditory impressions we can generalize that *all* sounds of the same pitches, C, E, and G—no matter how they are produced, by what instrument or voice—will be in an ascending order, provided they are produced in the original sequence. The universality of the tonal relations between C, E, and G is thus discoverable from a single instance. However, the justification of the generalization is semantic rather than logical, for the single experiential instance merely gives rise to the ostensive definition of the "pitch relation of sequential C, E, and G"; and we know from earlier discussions that *all* definitions—this one included—are universal. "Intuitive" induction, in other words, is simply a matter of definition. Any attempt to "prove" it logically presupposes the acceptance of a universal premise of some sort and assumes the validity of induction—the very point at issue.

Of course, judgments of sense impressions are not the only instances involving "intuitive" induction. Certain formal relations may be generalized in a similar way. Observing, for example, that 2 times 3 apples + 2 times 4 apples = 2 times (3 apples + 4 apples), we realize at once that $(2\times3)+(2\times4)=2\times(3+4)$; and since we also find that 5 times 6 pennies + 5 times 10 pennies = 5 times (6 pennies + 10 pennies), we generalize still further and assert that $n\mathrm{A}+n\mathrm{B}$

$=n(A+B)$, where n stands for any number, and A and B stand for any two homogeneous quantities. We have thus obtained the Distributive Law for Addition by a process of "intuitive" induction. Other laws of mathematics and of logic may be derived in a similar way. But back of all such "inductive" derivations and as their sole justification we always find definitions and semantic functions of mind.

(2) A different type of induction is encountered when we turn to "summary" induction or induction by complete enumeration (cf. Carnap, 1945). We may, for example, examine every book in a certain room and, in doing so, we may discover that each and every one belongs to Mr. M. When we have completed our examination we may assert that "All books in this room belong to Mr. M." This conclusion is justified on the basis of a syllogism of the following form:

Major premise: Books A, B, C, D, E are the property of Mr. M.
Minor premise: Every book in this room is identical either with A or with B or with C or with D or with E.
Conclusion: Every book in this room is the property of Mr. M.

Or, in generalized form:

Major premise: A, B, C, D, E are P.
Minor premise: Every (examined) S is either A, B, C, D, or E.
Conclusion: Every (examined) S is P.

It is obvious, of course, that not all cases of enumerative induction can be based upon *complete* enumeration. At times, the established conclusion, "Every (examined) S is P," will be expanded to read, "Every S is P." Such an "unlimited generalization," however, has ordinarily only a probability value. It attains certainty if, on the basis of the conclusion that "Every (examined) S is P," we define 'S' so that it implies 'P.' But in this case our induction is again of the "intuitive" type and the enumeration of instances was only a preparatory step. Why, then, enumerate instances at all? The answer is simple. An enumeration and examination of instances often leads to the discovery of new and unexpected qualities and conditions and may thus facilitate the proper definition of S.

The degree of probability of the proposition "All S is P"—if this proposition refers to more than the examined instances of S—depends upon the determinateness of 'S.' The more specifically 'S' can be defined, the more probable is the conclusion that "All S is P." But complete certainty of this conclusion cannot always be achieved, for

such certainty presupposes that the examined instances shall agree with one another only in those qualities which provide the basis for the definition of 'S,' and which therefore delimit the range of the generalization. Empirical objects, however, usually show similarities other than those which are essential to the definition of 'S,' and these additional similarities introduce an element of uncertainty into our "inductive" generalization.

At this point the real value of the enumeration of instances becomes apparent. The mere *number* of instances is irrelevant, but a number of instances also means a *variety* of instances and thus a maximum of difference. If the analysis of a few instances is insufficient for an understanding of their essential similarities, the examination of more instances, disclosing as it does a greater variety of unessential qualities, facilitates our understanding and leads to a more adequate and more specific definition of 'S.' The consideration of large numbers of instances is thus significant because of the possibility that large numbers entail a maximum of variation in unessential qualities, thereby disclosing more clearly what is essential in a given group of phenomena. It is therefore not surprising that the degree of certainty of our generalization is not proportional to the number of instances examined. It depends entirely upon the degree of determinateness which we achieve in our definition of 'S.'

If this is a correct interpretation of the meaning of "summary" induction, then we understand also the true significance of at least one phase of experimentation. 'To experiment' means to vary deliberately and under constant control the conditions under which a certain phenomenon occurs, and to do this in the expectation of separating the unessential from the essential properties and, in consequence, of obtaining a precise definition of 'S.' Behind all "enumerative" induction there looms thus as its goal a justification of the generalization through definition. What bearing statistical induction or "sampling" has upon this problem will be discussed in the next section.

(3) Let us now suppose that I am attempting a geometrical demonstration of the Pythagorean theorem. I first draw the appropriate figure and then show by means of previously established rules and theorems that the square of the hypotenuse is equal to the sum of the squares of the other two sides. Strictly speaking, however, I have demonstrated this relationship only for the figure used in the demonstration. To assert that the Pythagorean theorem holds for all comparable figures obviously involves an inductive generalization. This

generalization is justified, however, because the particular figure drawn on paper is subject to our demonstration only to the extent to which it embodies the qualities of triangles for which alone, and by definition, the Pythagorean theorem was intended to hold; and these qualities my figure must share with all "comparable" figures. That is to say, our generalization is justified because all accidental qualities of size and color of "lines," etc., are irrelevant to the meaning of 'triangle' for which the Pythagorean theorem holds. Our induction does therefore not really differ from the "intuitive" induction already discussed.

The situation is somewhat different in the case of so-called "mathematical" induction. The principle of this type of induction, as will be remembered, is that whenever a theorem is verified for some specific class from which all other classes of an orderly sequence can be built up by the use of accepted principles of construction, and if it can be shown that the progression from that class does not contradict the theorem, then the theorem is true for any class which may be derived from the first by the specified procedure. Hence, whenever a theorem can be proved for some specific whole number n and for $(n+1)$, it holds true for every whole number $\geqq n$.

(4) It is evident that the principle of "mathematical" induction serves only as a major premise in a demonstrative argument and that mathematical induction may therefore be regarded as a special form of "demonstrative" induction, and may be discussed under this heading.

"Demonstrative" induction is an attempt to arrive "demonstratively" at a conclusion which is more comprehensive or more inclusive than are the facts upon which it is based. It is the type of generalization which is meant ordinarily when a general reference is made to induction.

However, the "demonstration" of a generalized conclusion is possible only if we proceed from a universal and complex major premise. We may argue, for example, that

If some S is P, then every instance of S is P.
This S is P.
Therefore, every instance of S is P.

The validity of this syllogism depends entirely upon the truth of its major premise; and this premise, or any one of its equivalents, can be obtained only by a process of generalization, the validity of which

depends in turn upon the definitional determinateness of 'S'; for only if the *essential* nature of S entails P can it be justified at all without complete enumeration. "Demonstrative" induction, therefore, is impossible without prior "intuitive" induction. No matter how we view the situation, we cannot escape the importance of the definition of 'S' —especially when this definition is based upon the analysis of enumerated instances.

"Demonstrative" induction includes a sub-form which may be called "functional" induction and which plays an important part in the exact sciences. The scientist, as we have seen, aims at the correlation of phenomena by means of equations of the general form $E=f(C)$, where f stands for a specific type of dependency loosely called "causal," and where 'C' is the "cause" (the independent variable) and 'E' the "effect" (the dependent variable). If this equation holds for all the examined values of 'C' and 'E' of a given group of phenomena, then the scientist, as a rule, feels justified in asserting that it holds for *all* instances of the specified phenomena. His procedure involves an inductive generalization. However, since it is conceivable that many different interpretations of f would accurately describe the co-variations of any finite number of phenomena, the verification of the equation in a few instances does not in itself justify the claim that it is a general law.

Additional support for the generalization is obtained from the structure of the equation itself and from its relation to, or dependence on, other "laws" which are well established. Nevertheless, the real justification must again be found in the determinateness of our definition of 'C' and 'E,' and in the accuracy with which these definitions describe the essential aspects of the phenomena in question. Thus, "functional" induction, regardless of the "demonstrative" aspects which it involves, is ultimately grounded in, and derives its justification from, "intuitive" induction. The latter, therefore, is the real heart and essence of *all* induction; and since this is the case, it is obvious that there can be no "logic of induction" in the sense of rules and principles comparable to those which constitute the logic of deduction. Now, as always, the success or failure of an "induction" depends upon the "intuitive" or definitional grasp of what is essential in any given case.

It must be understood that the foregoing remarks about induction pertain only to forms of induction which do not involve statistical procedures.

Statistics and Sampling

In some respects the difficulties of induction can be reduced or even overcome through the employment of "statistical" methods. It is well known, however, that the mere collection of numerical data is insufficient and often misleading. Interpolations and interpretations are necessary if the collected figures are to disclose important facts and significant relations. "Averages," "median values," and "modes" may serve as indices of the "frequency distributions" of numerical data, but each device in itself has disadvantages as well as advantages, and no absolute reliance for all purposes is to be placed in any one or in all of these values.

The "average" or "arithmetic mean," for example, includes all of the items of a distribution, but it does not necessarily correspond to an actual item, nor does it give us an indication of the degree of "dispersion" of the data from which it has been computed. "Weighting" the averages may, in a measure, remedy the last-named deficiency; but in no case is it possible to analyze a given average so as to derive from it the component figures, "weighted" or otherwise, that entered into its computation. Averages are therefore misleading rather than helpful whenever they represent data involving a wide "scattering" and an asymmetrical distribution of the component items.

The "median," as a measure of position in a value scale, is a reliable index of distribution if the collected data are distributed with fair uniformity throughout the whole "range"; but when the distribution is badly "skewed," i.e., when the bulk of the items is at the top or the bottom of the scale or is divided between top and bottom, when, in brief, the distribution is far from uniform, the "median" is of little value. The "median" as a statistical device is most important when the data which it represents are closely grouped about the center of distribution. In all such cases, however, the "median" corresponds closely to the "average" and has the disadvantage that it cannot be manipulated algebraically.

The "mode" is the value which occurs most frequently in a collection of numerical data. It has little significance unless the total number of values is large, and unless the items involved are concentrated in a "cluster." But under favorable conditions it may be highly significant and may represent the true distribution of values more accurately than does the "median."

In order to indicate more specifically the distribution of items in a

scale of values, the scientist may compute the "mean deviation" from a determinate central tendency; and he may do so according to the formula $MD = \frac{\Sigma x}{N}$, where 'Σx' is the sum of all deviations and 'N' the total number of items; or, by dividing the "mean deviation" by the "mean" from which it has been computed, he may derive the "coefficient of dispersion" which discloses the relative proportion between "mean deviation" and "mean."

The "probable error"—a value which plays a most prominent part in the experimental sciences—is the amount of deviation which is exceeded by half of the items in a collection, measured relative to the "average" or "arithmetic mean" of that collection.

When it is desirable to stress the extreme variations in a dispersion, or when a "coefficient of correlation" is to be computed, a "standard deviation," derived according to the formula

$$\sqrt{\frac{\Sigma x^2}{N}}$$

is most useful.

If two or more sets of data are to be compared, this may be accomplished in some degree by comparing their respective "averages," "medians," "modes," "mean deviations," and "standard deviations." But such comparisons tell us little about the way in which individual items vary in the respective sets. A much more significant correspondence between the sets is obtained through the computation of their "coefficient of correlation," which discloses the interrelation of the "spreads" of the individual items about the respective "means" of the sets in question. This "coefficient of correlation," a pure number and not an index of some specific item, may range from $+1.00$ or perfect positive correlation, through 0.00 or no correlation, to -1.00 or perfect negative correlation; but it is usually some intermediate figure of a plus or minus value. It is computed in accordance with the equation

$$r = \frac{\Sigma(xy)}{N\sigma_x\sigma_y},$$

where '$\Sigma(xy)$' is the sum of the products of the deviations of corresponding items in the two sets, 'N' the total number of items in both sets, and '$\sigma_x\sigma_y$' the product of the "standard deviations" for the two sets.

Which correlations, as revealed by such computation, are significant and which are not is a somewhat debatable question. In some cases a coefficient of $\pm.45$ may be important; in other cases even a correlation of $\pm.65$ may not mean very much; but all statisticians agree that a correlation of .90 is much more than twice as significant as one of .45. There is danger also that too much emphasis is placed upon such correlations or that these correlations are interpreted in unwarranted ways. Correlations, after all, may be merely coincidental and the result of pure chance. They do not necessarily imply that the "correlated" sets stand in a direct causal relation to one another, for both sets may be the effects of a cause that is concealed by the correlation itself. A faulty arrangement of the statistical material prior to the computation of the "coefficient" may result in a substantial "correlation" when actually none exists. Time and the examination of larger numbers upset many "correlations"; and a prediction based upon a computed correlation always transcends the "evidence" and involves an "inductive leap" with all the dangers and logical uncertainties attached thereto. The chief value of correlations, therefore, lies not in their weight as proof (unless they are supplemented by other and stronger evidence), but in their usefulness during the exploratory stages of inquiry; not in their "explanatory" function (for they "explain" nothing), but in their "suggestiveness" which, when critically evaluated, may lead to the construction and development of testable hypotheses.

"Ratios" and "percentages" may also be useful statistical devices, but their employment is likewise beset with countless dangers. It is all-important, for example, to keep clearly in mind the "base figure" from which percentages are computed, and to distinguish carefully between "per cent of" and "per cent greater than." Percentages derived from very small numbers are particularly misleading, and the use of varying bases of comparison in one and the same computation breeds nothing but confusion. "Ratios" assume a homogeneity of the statistical material which frequently does not exist, and the general "frame of reference" within which percentages are calculated should never be omitted from the statement of the results.

But all this is of importance to the philosopher only to the extent to which it makes him reluctant to accept at face value every "conclusion" reached by statistical devices. Scientific procedure, in so far as it depends upon the use of statistics, is not necessarily the most reliable method for obtaining knowledge; nor is it self-corrective.

The discovery of "statistical regularities," however, when augmented by careful analyses of the data and procedures involved, is exceedingly significant. Observed "regularities," when properly interpreted, may disclose significant laws (e.g., the discovery of the Mendelian laws). If the regularities were not expected, they may reveal hidden connections which should be investigated further; and if they were expected but did not appear, their absence may indicate the influence of disturbing factors which might have been overlooked entirely but for the negative result of statistical procedures. What appear to be "statistical regularities" may, however, turn out to be no regularities at all when viewed in the "long run" or in connection with "larger numbers."

From the point of view of philosophy the most important statistical device is the procedure known as "sampling"; for this procedure provides us with a method of "induction" which, while not leading to absolute certainty, yields results of such a high degree of probability that they may well be regarded as dependable knowledge. The usual discussions of sampling procedures found in logic texts miss the most important point and become lost in insignificant generalities.

We shall here omit distinctions between "simple random sampling" and "stratified random sampling," and shall take for granted that the reader is familiar with the meaning of these terms. The only condition we must insist upon in all sampling is the "randomness" of the samples. 'Randomness,' as here understood, does not mean that the items which make up the sample are picked "at random" or "as they come"; it means, rather, that the conditions under which the samples are taken are controlled in such a way that all aspects (actual or possible) of stratification of the aggregate are represented in sample items taken at random.

If, under these conditions, several samples are taken from the same aggregate, each sample will in some respects differ from every other sample. These differences, the result of chance circumstances affecting the selection of individual items, are known as "random errors of sampling." If they are truly "random errors," they tend to cancel one another and thus to leave undisturbed the picture of the aggregate as a whole.

If the aggregate from which the samples have been taken is homogeneous, the random errors can be predicted with mathematical precision, and a curve can be drawn—the Gaussian or "normal" curve of errors—representing the theoretical random errors which the actual errors in an experimental sample more or less approximate.

If the results obtained by sampling depart markedly from the Gaussian curve, then either some basic error has been made in gathering or compiling the sample data, or the material involved is affected by "dominant causes" which disturb the random pattern. In either case a more careful analysis of the phenomena under investigation is called for.

We said a moment ago that *if the aggregate as a whole is homogeneous*, the "normal" curve of errors can be determined, and the errors of experimental samples taken from the aggregate will approximate that curve. But this is only a way of saying that if the composition of the aggregate (the frequency distribution of its differing items) is known, we can show that the composition of our samples approximates it within the limits of random errors.

The logic of this situation does not seem to change when we now reverse the point of view and when we draw an inference concerning the composition of the aggregate from the known composition of the samples. That is to say, if the nature or composition of the aggregate is unknown and random samples approximate a definite frequency distribution, it may be inferred from the samples that the frequency distribution in the aggregate is the same as that of the samples.

In this case, however, a number of factors may impair the inference, for the sample results are not always clear and concise representations of the aggregate. Random errors introduce small variations which blur the picture.

The reliability of any given sample depends in a measure upon the size of the sample. The smaller the sample, the more indefinite is the picture it provides. Its reliability varies as the square root of the number of items it includes. As the sample increases in size, each value represented in it tends to have the same relative frequency that it has in the aggregate; but the sample is a definitive representation of the aggregate only when it becomes so large that it is identical with the aggregate itself. The frequency distribution of values in the total aggregate is thus a "limit" which the distribution of these same values in the sample approaches as the size of the sample is increased. If several samples are taken from the same aggregate, the distribution of values in that aggregate is the "limit" toward which the distributions in all the samples converge.

The general ideas here briefly outlined have been given precise quantitative significance in the statistical theory of sampling—the theory which is the real object of our discussion.

Each sample can be regarded as an "array" or a frequency distribu-

tion, and the various averages (mentioned previously) may thus be computed for each sample in the same way as they are computed for any other array. Since, because of random errors, different samples taken from the same aggregate may vary, the arithmetic means of the samples may also vary and may differ from the arithmetic mean of the aggregate. But if they differ among themselves, the sample means may in turn be regarded as a frequency distribution and the arithmetic mean or average of this new distribution may also be computed. Once this has been done, the standard deviation of the samples from the sample mean can be determined; and if this deviation for the group of samples is low, the arithmetic means of the individual samples cluster compactly in a narrow range. In that case, any one of the samples may be regarded as representing adequately the aggregate from which all the samples have been taken. Additional samples will then not alter materially the picture of the aggregate so obtained.

However, if the aggregate from which the samples have been taken is not homogeneous, the sample means will not cluster compactly and the situation is not quite so simple. In this case it is necessary to compute the *standard error of the samples*. This magnitude is a measure of the variability of the arithmetic means of different samples taken from the same aggregate and is obtained for each sample from the standard deviation of that sample in conformity with the equation

$$\sigma_M = \frac{\sigma_s}{\sqrt{N-1}}$$

where σ_M is the standard error of the mean, σ_s is the standard deviation of the sample, and N is the number of items in the sample. σ_M measures the deviation of a given sample from the mean of all samples taken from the same aggregate.

The analysis of semi-interquartile ranges now shows that if normal distribution in an aggregate may be assumed, 68.27 per cent of all sample means fall within one standard deviation of the true mean of the aggregate, 95.45 per cent fall within the range of two standard deviations, and 99.73 per cent lie within three standard deviations (plus and minus) of that mean.

Let us now assume that the true mean of the aggregate is unknown but that it is the central value in an array of sample means. We then know that 68.27 per cent of all sample means fall within a range defined by the true mean plus and minus one standard deviation. We cannot assume, however, that any one given sample lies within this

range, for it may quite well fall outside the 68.27 per cent limit. We only know that the chances are 68.27 in 100 that it falls within the range defined. And we also know that the chances are 99.73 in 100 that it lies within a range defined by the true mean of the aggregate plus and minus three standard deviations.

If we now assume that the arithmetic mean of the sample lies three standard deviations *below* the true mean of the aggregate, the chances are 99.73 in 100 that the true mean does not lie more than three standard errors ($3\sigma_M$) above the sample mean. And if, correspondingly, we assume that the arithmetic mean of the sample does not lie more than three standard deviations *above* the true mean of the aggregate, the chances are 99.73 in 100 that the true mean does not lie more than three standard errors ($3\sigma_M$) below the sample mean. This, however, is only another way of saying that the chances are 99.73 that the true mean of the aggregate, although actually unknown, lies somewhere in a range of values defined by the sample mean plus or minus $3\sigma_M$.

Since the size of the sample, N, enters into the computation of the standard error of the sample, the standard error can be made increasingly small by increasing the sample. This means that the range defined by the limits of plus and minus $3\sigma_M$ can be reduced indefinitely. The computation of the standard error thus provides us with a significant test for certain "inductive" hypotheses—for those hypotheses, namely, which, on the evidence of sample distributions, ascribe to a given aggregate a specific frequency distribution of characteristic values. In this restricted sense at least, the statistical methods of sampling yield "inductive knowledge" which, while not absolute, is yet of a very high degree of probability.

Other devices—such as the computation of critical ratios—add to this certainty. But this is not the place to discuss statistical procedures in general.

Probabilities

A still different "statistical" procedure is involved in the computation of "probabilities." Recent discussions (e.g., the symposium, in three parts, in *Philosophy and Phenomenological Research*, 1945-46), have shown, however, that "probability calculation" is by no means the simple and unambiguous device it so frequently appears to be. The very meaning of the term 'probability' is not always clear and may mean different things in different contexts; and there is consid-

erable divergence of opinion as to what can and what cannot be determined by a calculation of probabilities. The issues involved should concern the philosopher at least as much as they do the scientist, for they are epistemological rather than mathematical and ultimately they involve the whole logic of cognition.

In order to clarify the problem we shall distinguish between three theories of probability (which we shall call "probability$_c$," "probability$_f$," and "probability$_t$," respectively), and shall leave open, for the time being, the question of a possible reduction to two basic types or to one.

(1) "Probability$_c$," as here understood, is the *classical* theory usually associated with the names of Laplace, DeMorgan, Boole, DeFinetti, Castelnuovo, and others—the theory which Professor Williams brought into the forefront of discussion in the symposium referred to above. It is the theory according to which 'probability' means a measure of objective certainty associated with a belief, a measure of "credibility" or "worthiness to be believed" when no absolute certainty can be obtained. Such "probability" must not be confused with "subjective expectancy" or with more or less accurate "hunches"; for it is assertable only on the basis of objective evidence, i.e., on the basis of evidence which is accessible to all and available for quantitative analysis. Typical examples of this kind of "probability" are the "probability of $\frac{1}{2}$" that a "normal" coin, when flipped, will fall head up; or the "probability of $\frac{1}{6}$" that the throw of an "unloaded" die will result in a "five."

When we analyze these examples, the characteristic features of "probability$_c$" become evident. There is, first, the fact that this probability is a *ratio* of all *favorable* instances to all *possible* instances, i.e., it is the ratio $\frac{f}{f+u}$. There is, secondly, the fact that this ratio can be determined prior to all "throws" or empirical "occurrences," and that it can be determined from an analysis of the objects involved. The die, for instance, has a total of six sides, and only one side bears the markings signifying "five"; its ratio of $\frac{f}{f+u}$ is therefore $\frac{1}{6}$. Underlying such an analysis, however, are certain presuppositions which belong to the logic of the calculation itself and which cannot be neglected in an evaluation of the procedure.

The most basic of these presuppositions is the requirement that the *possible* instances are in each case *exclusive and exhaustive alternants,*

that the occurrence of one precludes the simultaneous occurrence of all others, and that no additional alternatives are conceivable. The next presupposition, and one equal in importance to the first, is the requirement that in each case the exclusive and exhaustive alternants are *equally* possible, and that there is neither an a priori nor an empirical reason why one should occur more frequently than any other. Both requirements, individually and together, entail the ideality of the basis of calculation for "probability$_c$"; for in empirical cases we can never be certain that all alternatives have been stated, nor do we know for sure that all alternatives are equi-possible. In the case of the coin, for example, which is assumed to fall either heads up or tails up, we disregard the additional possibility of its standing on edge; and in assuming the equi-possibilities of heads and tails we disregard, furthermore, any minor flaws which may affect the actual occurrence of heads or tails over a long period without being so marked as to prove the coin to be not "fair."

The ideality of the basis of calculation is, of course, no specific characteristic of "probability$_c$." We encounter it everywhere throughout the whole realm of science. Newton's second law of motion ($F = kMa$), for instance, completely disregards all frictional resistance and assumes that both mass and acceleration are given as completely determinate values; and the first law of thermodynamics, implied as it is in the definition of the mechanical equivalent of heat ($ME = J = \frac{W}{H}$ = "work units" divided by "heat units"), assumes the existence of an "isolated system" from which no heat can escape. Other laws of science involve corresponding assumptions; and in this sense, as we have seen in the preceding chapter, *all* laws deal with "idealized" cases. The logical relation between the number of alternatives assumed in the calculation of probabilities is therefore, in principle, no other than that of any law of science to the phenomena with which it is concerned.

The applicability of "probability$_c$" is, however, greatly restricted by the requirements that (a) the probability must be statable as a ratio, and that (b) the basis for its calculation must be obtained from an analysis of some object representative of the class for which the calculation is to be made. The first of these requirements makes the application of "probability$_c$" impossible where the "chances" are statable only as irrational numbers; while the second requirement restricts its use in broad fields of science where the analysis of repre-

sentative objects is impossible and where we depend entirely upon the empirical evidence of "frequency distributions."

"Probability$_c$" has the advantage, however, that wherever it is applicable it can be employed in dealing with individual cases. It has thus a certain "predictive" value, the logic of which is evident from the principle of Peirce's "statistical syllogism"; for if $\frac{\text{'m'}}{\text{n}}$ designates the "probability ratio" that a member of class '*M*' has the property of '*P*,' and if *a* is a given member of *M*, then the probability that *a* possesses *P* can be inferred from the syllogism

$$\frac{\mathrm{m}}{\mathrm{n}}\, M \text{ is } P$$
$$\mathrm{a} \text{ is } M$$
$$\text{Therefore, } \frac{\mathrm{m}}{\mathrm{n}}\mathrm{a} \text{ is } P$$

If the "probability ratio" is 1/6 that the casting of a "normal" die will result in a "five," and if I now cast a "normal" die, then the probability is 1/6 that I now obtain a "five."

The chief criticism of "probability$_c$" centers around the requirement of equipossibility for all stated alternants; for the only criterion of equipossibility offered by the classical theory is the "principle of insufficient reason" or of "indifference," according to which two events are to be regarded as equally possible if there is no known reason for supposing that one of them will occur more often than the other. This "principle of ignorance" (as it also has been called) does seem to be a serious deficiency of "probability$_c$," for our "ignorance" of causes and conditions is a poor legislator for the facts; but is it actually the kind of deficiency it is so frequently made out to be? Let us inspect more closely the "statistical syllogism."

The *major* premise defines a probability ratio $\frac{\mathrm{m}}{\mathrm{n}}$ for certain ideal situations in the very same sense in which any law of science determines specific functional dependencies for certain other ideal situations. And the way in which, for example, the probability ratio of $\frac{1}{6}$ for a "normal" die can be derived from the geometrical structure of a cube differs in nothing from the manner in which Galileo obtained his law of falling bodies from an analysis of "idealized" uniform and accelerated motions. The crux of the matter, however, is the *minor* premise; for it asserts that *a* is indeed a member of the (idealized)

class *M*. The minor premise, in other words, contains the "applicative principle" of the syllogism; and the argument as a whole is valid only to the extent to which the minor premise is true. But this again is the same as it is in the case of the functional laws of science, and the logical consequences are identical. We shall return to this point shortly.

(2) At the very point where "probability$_c$" is weakest, "probability$_f$" seems to achieve its greatest success; for "probability$_f$" is concerned primarily with actual and empirically determined "frequency distributions," not with alternants assumed to be equipossible. As first formulated and as developed by Bolzano, Cournot, Venn, von Mises, Ernest Nagel, and others, "probability$_f$" is a measure of the relative frequency with which a property occurs in a specified class of elements. An example may make this clear.

According to the Institute of Actuaries, the probability that a man of 40 residing in the United States shall live to be 50 is .884. That is to say, approximately 884 out of a thousand forty-year-old men in the United States will live for at least another ten years. That this probability is not obtained through the employment of the classical formula $\frac{f}{f+u}$ is obvious; for to maintain that it has been derived from an analysis of 1000 equipossible alternants found in connection with some "representative" man is to maintain an absurdity. How, then, was "probability$_f$" obtained?

The Institute of Actuaries used a table giving all the ages from 10 upwards and, beginning with 100,000 persons alive at the age of 10, placed opposite each succeeding age the number of survivors, until at age 98 none was left. At age 40, there were 82,284 survivors; at age 50 there were only 72,726. The ratio of survivors at 40 and 50 is therefore 82,284:72,726 or 1:0.884. Strictly speaking, the evidence given warrants only the statement that *of the cases examined* the men aged 40 had a chance of .884 of being alive at 50.

But let us assume now that the Institute of Actuaries actually examined in a similar manner three or four other groups of 100,000 persons, each picked at random from different parts of the country, and that the results showed in these cases that the men aged 40 had a chance of .8843, .8839, .8841, and .8838, respectively, of living to age 50. The conclusion drawn from the first sample would then be materially strengthened. And if we now *assume* that the conditions determining the observed ratios of survival will prevail for an *indefi-*

nite number of years to come, then the "probability$_f$" that a forty-year-old man living in the United States has a chance of .884 of surviving at least until he is 50, means that in the long run the ***relative frequency*** with which forty-year-old men in the United States live at least another ten years is approximately .884. If interpreted in this way, "probability$_f$" entails a prognosis. It is a *hypothesis,* based upon empirical evidence, from which, as a partial premise, certain consequences may be deduced (such as the cost of life insurance at age 40) which may be put to observational tests. But "probability$_f$" does not entail a prognosis concerning an individual person or object, nor even concerning any given 100 or 1000 persons or objects. It is concerned exclusively with frequency distributions involving large numbers and a "long run" perspective.

Most probability statements in the natural sciences are of this type. The scientist, however, demands precision and univocality of statement—especially when he deals with quantities and with the manipulation of quantities. "Probability$_f$," on the other hand, as defined above, involves vague and ambiguous terms—such as "approximately," "in the long run," and "large numbers." A strict mathematical interpretation of "probability$_f$" must avoid such vagueness and must employ precise concepts instead. This can be accomplished through recourse to the mathematical notion of limits.

Let R be a (non-empty) "reference class" under investigation. If this class is finite, the order of its elements is immaterial; if it is infinite, its elements must be serially ordered, for otherwise no "limits" can be obtained. Let P be some property or attribute which the elements of R may possess. Let n be the total number of elements in R, and $\mathrm{m}(P \text{ and } R)$ the number of elements in R which actually do possess P. Corresponding to the formula of "probability$_c$," $\frac{\mathrm{f}}{\mathrm{f}+\mathrm{u}}$, we can then express "probability$_f$," or the relative frequency with which elements in R possess P, by the formula $\frac{\mathrm{m}(P \text{ and } R)}{n}$. "Probability$_f$" is thus a proper fraction. But, as is evident from the formula, the value of this fraction changes with changing n. In general, however, the different values of the fraction will center around some fixed number p_f. They will differ from p_f by a small amount ϵ; and this amount will diminish as n increases. The fractions determined by the variations of n, in other words, tend toward p_f as their *limit*; and "probability$_f$" may therefore be defined as *the limit of relative frequency*:

$$p_f = \lim_{n \to \infty} \frac{\mathrm{m}(P \text{ and } R)}{n}$$

If this interpretation is accepted, then the "probability$_f$" that a forty-year-old man residing in the United States will survive at least another ten years is .884 must be taken to mean that *the limit of the relative frequency* with which the property of surviving at least another ten years occurs in the ordered class of forty-year-old men residing in the United States is .884. The mathematical manipulation of limits involves nothing new (cf. the preceding chapter).

Several comments are in order.

(a) The discussion of "probability$_f$" clearly involves two distinct aspects. (i) It refers to the statistical determination of the relative frequency of a property P in a (non-empty) "sample class" C. (ii) It goes beyond the actual evidence pertaining to the "sample class" and leads to a definition of the "complexion" of a (possibly infinite) "reference class" or "collective" R. In making this transition, the statement of an actual "frequency distribution" is changed into a "probability prognosis" for the "collective." Both aspects, (i) and (ii), I believe, must be clearly separated; for (i), being merely a matter of collecting statistical data, involves no problem of interest to the philosopher, whereas (ii) is an entirely different matter and involves several problems of significance for logic and epistemology.

(b) The determination of the "complexion" of the reference class depends upon an *inductive interpretation* of the sample classes. "Probability$_f$," therefore, assumes as its *sine qua non* the validity of some process of induction. It has its indispensable supplement in the statistical procedure of sampling (cf. the preceding section).

(c) The "complexion" of the reference class or collective, defined as the limit of relative frequency for P in R, is an *idealization*; and this for two reasons. (i) The induction employed in its definition depends upon sufficiently long series or sufficiently large numbers of observations to indicate a definite trend; but what are "sufficiently long series" or "sufficiently large numbers" is a matter of practical decision. This means, ultimately, that we *posit* or *stipulate* that "in such and such case *this* is sufficient." (ii) The derivation of "limits" is in itself an idealizing procedure. "Probability$_f$" is therefore just as much bound up with "ideal" cases as is "probability$_c$."

(d) The principal charge against "probability$_c$" was that it presupposes and depends upon the "principle of indifference," according

to which exclusive alternants may be assumed to be equipossible if no reason is known why one should occur more often than another. "Probability$_f$" presupposes and depends upon a comparable principle; for it assumes that the sample classes represent *random* samples of R, and "randomness" here means, as we have seen, that the samples are so taken that, presumably, any one item has exactly the same chance of being chosen as any other. The samples, in other words, are so taken that there is no known reason why one item should be preferred to any other.

(e) In discussing probabilities, a distinction must be made between *events* or *items* and *kinds of events* or *classes of items*. "Probability$_c$," through the employment of the statistical syllogism, provides prognostic hypotheses for individual events or individual items. The "probability" that some particular flip of a coin will produce "heads" is thus $\frac{1}{2}$. "Probability$_f$" on the other hand, is said to pertain to classes of events or classes of items only. Most "frequentists" explicitly repudiate the idea of a probability for single cases and allow only elliptical references to individual events. So long as a probability statement is simply an assertion concerning the relative frequency with which a *kind* of event (or item) occurs in some specified reference class, it is obvious that it cannot strictly apply to an individual event (or item); for an individual event (or item), literally speaking, can have no relative frequency in R. Individual persons, for example, have no relative frequency of surviving. Human expectations, however, are most frequently concerned with individual events (or items), and with probabilities concerning their fate. "Probability$_f$," therefore, in so far as it is concerned exclusively with *kinds* of events, remains psychologically unsatisfactory.

(f) Every "probability statement" is a hypothesis and cannot be completely verified by a finite amount of evidence. This is true for "probability$_c$" no less than for "probability$_f$"; but in the case of "probability$_f$" the assertion is often ambiguous. It may refer (i) to the fact that the "complexion" of R, defined as the limit of relative frequency of P in R, is a hypothesis; for the specific limit is in every case the result of an induction and is therefore subject to future correction. Or it may mean (ii) that the "complexion" of any given R is taken for granted and is employed as a prognostic hypothesis for individual events (or items) in much the same way in which the fixation of exclusive alternants of "probability$_c$" is taken as such a

hypothesis. If (i) is the case, "probability$_f$" means that the relative frequency of P in R can be known only with an "inductive probability" which in itself depends upon factors other than frequency distributions. "Probability$_f$" in this case is a theory of relative frequencies and involves only the general problem of induction. It may be well, in this case, to discard the term "probability" altogether and to speak of relative frequencies only. But if (ii) is the case, then "probability$_f$" plays essentially the same role that is usually assigned only to "probability$_c$," and the question arises, Can such an interpretation of "probability$_f$" be justified?

(g) Let us assume that through inductive procedure the "complexion" of an (idealized) reference class has been determined to our satisfaction. For example, the limit of the relative frequency with which the property of surviving at least another ten years occurs in the ordered class of forty-year-old men residing in the United States is .884. How does this affect the probability that Jack Robinson, a forty-year-old man residing in the United States, will live to be at least 50 years old? If the "complexion" of R can be identified with the "probability ratio" of P in R, then this ratio can be used in the major premise of the statistical syllogism, and the argument, as applied to Jack Robinson, is obvious; for the "probability" that he will live to be 50 is then numerically identical with the "probability ratio" of P in R. But is the "complexion" of R a "probability ratio"?

That the "complexion" of R is logically as well as factually the same as the (idealized) "ratio of cases" of P in R is beyond question; but is this "ratio of cases" a "*probability* ratio"? If it is not, then the statistical syllogism cannot be employed when we deal with frequency distributions, and "probability$_f$," strictly speaking, is not a probability. If it is, then the difference between "probability$_f$" and "probability$_c$" disappears at this point. There is left as a distinguishing mark only the difference in the procedures by which the probability ratios are determined in each situation; and this is a minor distinction which entails no far-reaching consequences.

Most objections to the identification of "ratio of cases" of P in R with "probability ratio" seem to stem from the fact that in some instances the interpretation of a "complexion" as a "probability ratio" may involve or lead to semantically undefined terms. I believe, however, that the difficulties here referred to are more apparent than real, and that they can be eliminated without much trouble. In the example

given above, for instance, we can readily state the results in an argument such as this:

> If the "probability ratio" is .884 that forty-year-old men residing in the United States will live at least to the age of 50, and if Jack Robinson is a forty-year-old man residing in the United States, then the probability is .884 that he will live to the age of 50.

And this is in all essentials the statistical syllogism of "probability$_c$" as applied to a "probability ratio" derived through an inductive interpolation of statistically determined relative frequencies in *R*.

(h) I do not contend that the preceding remarks have eliminated all difficulties encountered in probability theories. I believe, however, that the issues have been clarified in some respects. (i) Probability, in the sense of a prognostic hypothesis concerning individual events (or items), is one thing; the distribution of relative frequencies in *R* is quite another. (ii) "Probability$_c$" is concerned with the former, whereas "probability$_f$" pertains essentially to the latter and to this extent might better be called by a different name, such as "theory of relative frequencies" (cf. Williams). (iii) In so far as the "ratio of cases" of *P* in *R* is acceptable as a major premise in the statistical syllogism, i.e., in so far as the "ratio of cases" can be interpreted as a "probability ratio," "probability$_f$" has exactly the same logical significance as has "probability$_c$," and is determinable in the same way. (iv) In view of (iii), the difference between "probability$_c$" and "probability$_f$" is but a difference in procedure in discovering or determining the "probability ratio." "Probability$_c$" relies primarily upon an antecedent analysis of some representative case; "probability$_f$" depends upon a statistical compilation of relevant data. (v) Both approaches necessitate an "idealization" of the cases under investigation and lead to definitions of the "probability ratio" for idealized situations. (vi) Both interpretations yield hypotheses which may or may not be "confirmed" in actual trials or through prolonged observation. (vii) Both are equally "empirical," but are not more so than any of the functional laws of the natural sciences. (viii) Excluding the "theory of relative frequencies" from further consideration, it seems that the general and essential parallelism of "probability$_c$" and "probability$_f$" can best be recognized by fusing both in a broader conception, "probability$_{fc}$," which recognizes as equally legitimate the different ways of determining the "probability ratio," but which employs the statistical syllogism as does "probability$_c$" and does so for the same

purpose. (ix) "Probability$_{fc}$," in other words, provides a prognostic hypothesis concerning the fate of individual events or cases. (x) In providing this hypothesis, "probability$_{fc}$" furnishes an objective measure of certainty of human expectations, and an objective basis for the "credibility" of a belief. And this is the most that can be expected of any theory.

(3) We turn, finally, to a consideration of the third type of probability theory—the type previously identified as "probability$_t$."

This type of probability involves statements such as these: "The available evidence makes it highly probable that man lived in Nebraska at least ten thousand years ago"; "Considering all known facts, it seems improbable that Americans will ever return to complete isolationism in international affairs"; "Relative to the evidence available at present the theory of quantum mechanical resonance in chemistry has a probability of being true which is greater than it was relative to the evidence presented in 1938"; "In view of the facts known today, the theory of evolution is more probable than the theory of special creation."

The "probability" referred to in these statements obviously pertains to the *degree of adequacy of the evidence* supporting each assertion, i.e., it pertains to the *degree of confirmation* of stated hypotheses or, if you prefer, to the *weight of evidence*, favorable and unfavorable, which is relevant to a given hypothesis. It is, in brief, the "truth-probability" of a hypothesis.

Hans Reichenbach, in particular, has concerned himself with this kind of probability; and in the discussion which follows, his views provide the background for our analysis.

No hypothesis can ever be completely verified through a finite number of observations, for verification through observation involves inescapably the fallacy of affirming the consequent. However, a hypothesis can be "confirmed" in various degrees by observational evidence—provided the hypothesis, h, and the evidence, e, are logically related in a specific manner. A hypothesis is *capable of being* "confirmed" only if it *entails* propositions concerning the *kind* of evidence that is relevant to it; i.e., h is capable of being "confirmed" only if the statement h entails the specifications of e. "Material" or "strict" implication of e by h is not sufficient (see the next section). A hypothesis *is actually* "confirmed" (as distinguished from being "capable of confirmation") if specific instances of e have been observed. Often, however, observation reveals *negative* instances of e

as well as *positive* instances; and the "degree of confirmation" of h then depends upon a proper evaluation or relative "weighting" of the positive and negative e.

For the sake of simplicity we shall assume that for a certain h there is no negative e; i.e., we shall assume that the evidence disclosed in observation is all positive; and we shall exclude from consideration all other factors and conditions which might complicate matters. Let us now suppose that we put h to a test and that we obtain e_1 as the first evidence "confirming" h. As we continue the process of testing h, we accumulate additional instances of e, say, e_2, e_3, and e_4. It is true, in a general way, that h becomes more and more securely established as the instances of e accumulate. The "weight" of the evidence is, in this sense, a function of the number of positive instances of e. The "truth-probability" of h therefore seems to increase with increasing e.

It must be admitted, however, that an accumulation of positive instances of e does not greatly strengthen h—*if all of them are of the same kind*. If, in the case under consideration, observation were to reveal an e_5 totally different in character from the previous instances e_1, e_2, e_3, and e_4, the degree to which e_5 contributes to the probability of h might well surpass by far the cumulative effect of e_2, e_3, and e_4 (which are all of the same kind as e_1); and the discovery of an instance e_6, if it is a so-called "crucial" instance, may easily outweigh all instances e_1 to e_5. All of which means that increasing confirmation of h is not simply a matter of cumulative instances of e. The *difference in kind* of instances of e has, in general, a greater bearing upon the "truth-probability" of h than has the mere numerical increase of positive instances. As a consequence, the "weight of evidence" cannot be arranged in the simple linear pattern of an ordered series; and this means that the increasing degrees of confirmation cannot be quantized. It is, therefore, not subject to mathematical computation.

We have reached this conclusion from an analysis of the simplest case of $h \rightarrow e$; for we have admitted only instances of positive e and have ruled out all complicating factors—such as rival hypotheses and the interrelation of h with an accepted body of knowledge. Under the actual conditions, therefore, under which most scientific hypotheses are tested, the difficulties of quantizing the "weight of evidence" increase immeasurably and preclude all chances of success.

A decision to assign different numerical "weights" to the various kinds of (favorable and unfavorable) instances of e does not help

matters much; for such assignments are arbitrary and depend entirely upon subjective evaluations. They find no justification in "relative frequencies" (for they have no connection with frequencies), and very little in antecedent analyses of expected *e*. What "weight" is to be given any particular instance of *e* remains, as before, a matter of "practical" decision—a decision guided in all essentials by the experience of the expert in his field and by the general reliability of scientific method.

"Probability$_t$," therefore, is quite different from "probability$_{fc}$" or any one of its components. It has nothing to do with numerical ratios or with precise computations of probabilities. It designates a subjective attitude toward *h* rather than a well-defined objective basis of credibility of *h*. And in this sense it belongs to the realm of pragmatics rather than to that of logic and epistemology.

The Use of Hypotheses

Observation, measurement, inductive generalization, and statistics —the aspects of scientific procedure so far discussed—by no means constitute the whole of scientific method. In a sense, they are preliminary steps or "tool devices" for the real task of science; while the computation of probabilities indicates a first advance beyond the mere collection of data. The ultimate task of the sciences, however, is the solving of problems and the functional integration of experience.

Now, problems arise in daily life whenever our desires or volitions are frustrated, or whenever our complacency is disturbed by the unexpected presence or absence of certain phenomena, or whenever unexpected changes take place in familiar objects or situations. The problems with which scientists deal grow out of the problems which confront us in everyday living. However, in consequence of centuries of scientific tradition, the scientist no longer waits for the problems to arise in haphazard fashion; he looks for them and forces them into prominence through experimental disturbances of the "normal" course of events. Many of his problems are the result of his own theories and are suggested to him by his own imagination. Others he creates by deliberately modifying the conditions under which given phenomena occur. Still others are entailed by solutions of problems thus found or created.

But whatever the origin of the problem, the indispensable first step on the road to its solution is a more or less clearly conceived idea that there *is* a problem and that it can be reduced to a specific question.

Once the scientist knows what this question is, he can make a "guess" (or a number of "guesses") as to a possible answer. The "guesses" he makes are the *hypotheses* which either "solve" the problem or guide him in further investigations.

The formation of a "guess" or hypothesis is, of course, an act of creation. Analogy may be a psychological aid, but ultimately the scientist depends upon his own creative imagination. Experience in the field and a comprehensive knowledge of accepted laws and theories are of inestimable value in the formation of new hypotheses; but there is no logical or methodological device that can be substituted for insight and understanding; and no rules can be given for the process of formulating hypotheses. Certain requirements of the hypotheses themselves, however, may be noted and these requirements are given in every elementary textbook on logic.

Any "guess" which is to be acceptable as a possible solution of a problem must at least be free from logical contradictions. In order to be acceptable as a *scientific* hypothesis it must also be physically possible and must not transcend the realm of physical reality in the sense of appealing to some "supernatural" agency. In general, a hypothesis should be in harmony with the best authenticated knowledge available in the specific field of inquiry and should not conflict with established laws. This last requirement is not an absolute condition, however, for a new hypothesis, if adequately confirmed, may lead to a complete revision of hitherto accepted theories and laws. Modern quantum mechanics, for example, was developed in opposition to the accepted principles of classical physics—notably in opposition to the principle that all processes in nature are continuous.

One of the basic requirements of a good hypothesis is the demand that it entail a number of consequences which can be tested experimentally; for only if a hypothesis can be tested can it ever be confirmed, and only if it can be confirmed does it amount to more than a "guess" and does it attain standing in the world of science.

It has been said that one of the principal tests of a good hypothesis is that it makes prediction possible. While this is true in general, it must be noted that it is not the prediction as such which is decisive. The weight of a prediction itself depends upon whether or not it could have been made without this particular hypothesis. In other words, it depends upon whether or not the hypothesis in question is the only one which, under the circumstances, entails the predicted event. Since we are never sure that only one hypothesis can provide

the solution of a scientific problem, all scientific knowledge remains subject to revision. It can attain only a high degree of probability (in the sense of "probability$_t$"), never absolute certainty.

It is evident from our discussion of "probability$_t$" that in evaluating the "weight" of evidence we must rely upon the judgment of an expert in his field or upon the consensus of experts dealing with related problems. Their "practical" decision that such and such evidence is sufficient (or insufficient) to warrant the acceptance of a given hypothesis as "confirmed" is the only authority we have. But what is to guide these experts? And why should we accept their judgment?

The latter of these two questions is easily answered, for their specialized knowledge in the field and their familiarity with the methods of research put the experts into a most favored position for such evaluations. Nevertheless, we accept their decisions only if, in the light of available evidence, they seem to be warranted. That is to say, we accept their decisions because we trust their judgment in evaluating the evidence more than we trust our own; but we insist upon the evidence. And it is the evidence which guides the experts. How is this to be understood?

To begin with, the logical structure of the proposed hypothesis must be carefully analyzed. Logical flaws in the hypothesis itself militate against its acceptance and may disqualify it altogether. Attention must be paid not only to the internal consistency of the hypothesis itself, but also to the definitions and specifications which determine the factual conditions under which it is to be verified. If the entailed consequences of a (logically acceptable) hypothesis are contradicted by the facts of observation, the hypothesis itself is disproved or falsified. The logical argument of such falsification takes the form of a valid hypothetical syllogism in which the categorical minor premise denies the consequent. A single experiment may therefore disprove or falsify a hypothesis—unless we decide for reasons of systemic completeness that the observation was faulty, misinterpreted, or distorted by unexpected factors or conditions.

But if the facts of observation confirm the consequences of a hypothesis, the logical argument of such confirmation takes the form of an invalid hypothetical syllogism in which the categorical minor premise affirms the consequent. A single experiment can therefore never "verify" a hypothesis; it provides at best only "confirmatory" evidence. This evidence is strengthened whenever the hypothesis in

question entails various and mutually independent consequences which all find independent confirmation. For example, Newton's hypothesis that sunlight consists of a mixture of rays differing in refrangibility, and that the different colors of the spectrum correspond to the different degrees of refrangibility, entails that rays of different colors cannot come to a focus at the same distance from the lens through which they pass; that for each color there must exist a definite and specific amount of refraction; that the refrangibility of every color is constant; that "white" light can be produced by "mixing in due proportion" all the primary colors just as the colors of the spectrum can be obtained by "breaking up" the "white" light into its component rays; that the "permanent colors of natural bodies" must be explainable in terms of reflection of light rays, and so on. Through a series of ingenious experiments Newton verified every one of these consequences of his hypothesis, and it was the *cumulative* weight of the evidence which established his hypothesis as "highly probable." The phrase "highly probable" was chosen advisedly, because the formal proof falls short of its mark even now (for a somewhat different view see Dubs, 1930).

In the case of Newton's hypothesis none of the entailed consequences was contradicted by observation. Should some other hypothesis be confirmed in some respects and falsified in others, its "truth-probability" would be weakened to that extent, and a revision of the hypothesis or of the evidence would be necessitated by the facts.

That a good hypothesis may, at times, fail to be confirmed because of faulty evidence can readily be demonstrated from the history of science. When Newton first formulated his "law" of gravitation—to refer to only one example—he failed to verify it because he had at his disposal erroneous estimates of the size and distance of the moon. When the astronomers corrected their estimates, a hypothesis which had been discarded as "falsified" found ample confirmation and became one of the basic laws of classical mechanics.

In general, however, a hypothesis is not abandoned merely because its consequences are only *approximately confirmed* by observation; and this for two reasons. (1) No empirically testable prediction is ever made on the basis of a hypothesis alone. It depends also on statements of "initial conditions"; and these as well as the tests and measurements employed to verify the hypothesis are subject to errors of observation. The scientist must therefore allow for certain deviations from the predicted values. This is true in particular since his

hypothesis is formulated for "idealized" cases rather than for actual situations in which disturbing factors may abound. What "margin of error" he is willing to accept depends upon the nature of the problem and upon the precision and general reliability of his instruments and methods of analysis.

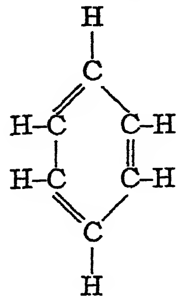
(2) Any given hypothesis may (and frequently does) presuppose laws and hypotheses which themselves are only partly confirmed or which are only approximately true. If this is the case, it may be necessary to alter the presuppositions before consequences can be obtained which find confirmation in observation. Through such alterations an otherwise unconfirmable hypothesis may be saved.

Furthermore, the evidence which supports or verifies a hypothesis does not always consist of facts which directly confirm its consequences. A hypothesis may also be confirmed by the bearing it has upon other hypotheses or upon established theories, particularly when it links these theories and hypotheses together into one all-inclusive system. Einstein's general theory of relativity, for example, has not only been confirmed by direct evidence, but has been shown to be one of the great synthetic ideas of modern physics and, as such, has found confirmation which far surpasses the experimental facts which confirm its specific consequences.

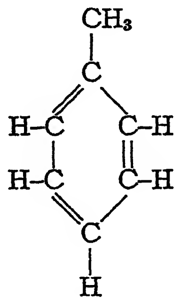
In general it may be said that a hypothesis is confirmed to the degree to which it performs the task for which it was invented. It is even more acceptable if, beyond its original purpose, it accounts for phenomena not otherwise explainable or not explainable in the same coherent and systematic fashion.

Let us now consider a specific case of the use of hypotheses. We select an example from the field of chemistry.

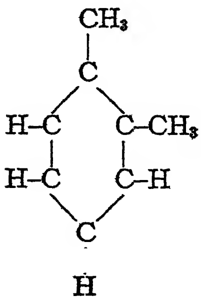
Let us assume that we are examining the principal hydrocarbons commercially available from the processing of coal tar, and that we have arranged them in the order of increasing boiling points. The first compound on our list is benzene (boiling point 80°C); the next is toluene (boiling point 111°C); and the third is xylene (boiling point somewhere between 138°C and 144°C). The indefiniteness of the boiling point of xylene suggests as an obvious problem the question, What is the reason for this indefiniteness? In order to find an answer to this question, we now examine the (theoretical) structure of the compounds. We find that the structure of benzene—C_6H_6—can be represented by a simple Kekulé ring:

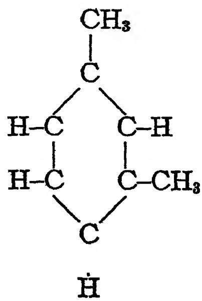


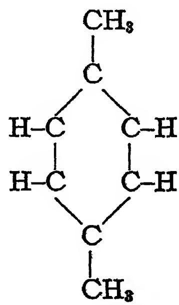
The structure of toluene—$C_6H_5(CH_3)$—can be represented by a similar ring in which the CH_3 group replaces one H of the hydrogen atoms, thus:



When we analyze the structure of xylene—$C_6H_4(CH_3)_2$—we discover, however, that the presence of two CH_3 groups makes possible three distinctive arrangements of the various groups in the ring, namely:

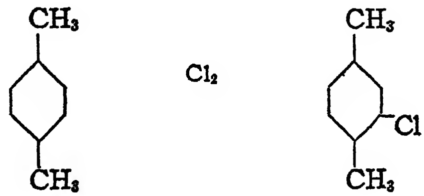
I: 

II: 

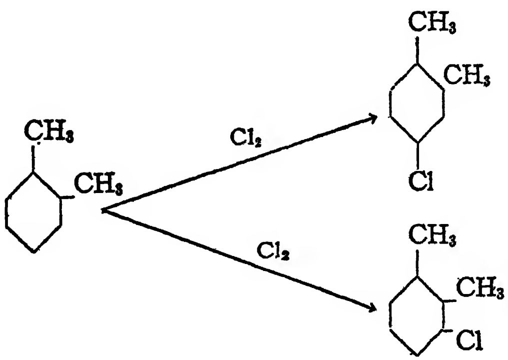
III: 

the ortho-, meta-, and para- forms, respectively. We therefore formulate the hypothesis that xylene, as ordinarily obtained from coal tar, is a *mixture* of three distinct and structurally different forms.

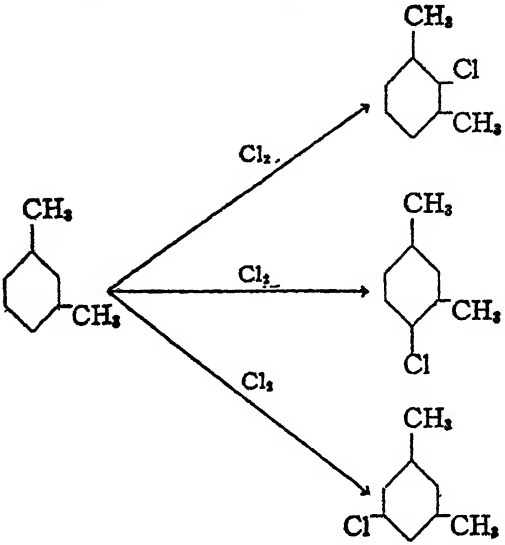
This hypothesis entails specific differences in the reactions of various substances with the three forms of xylene. Reaction with chlorine, for instance, entails structurally the following distinctions: In the case of para-xylene one and only one chemically identifiable compound, $C_6H_3(CH_3)_2Cl$, is possible, because in the para- form all CH groups are in their respective places in identical position relative to the two CH_3 groups; hence,



In the case of ortho-xylene two and only two chemically distinct compounds, $C_6H_3(CH_3)_2Cl$, are possible and both differ from the compound derived from para-xylene. This is so because in the ortho- form all but two CH groups are in their respective places in identical positions relative to the CH_3 groups; hence,



In the case of meta-xylene, finally, three and only three chemically distinct compounds, $C_6H_3(CH_3)_2Cl$, are possible, and all three differ from the compounds derived from para- and ortho- forms. The reason for this is, as in the preceding cases, the specific structure of meta-xylene; for we find without duplication of structure that



The theoretically anticipated reactions actually do occur. Our hypothesis that ordinary xylene is a mixture of three distinct forms of xylene has thus been confirmed.

If we now isolate these forms of xylene and examine their respective boiling points, we discover that ortho-xylene has a boiling point of 144°C, that meta-xylene has a boiling point of 139°C, and that para-xylene has a boiling point of 138°C. The indefiniteness of the boiling point originally observed is now fully explained by the (confirmed) hypothesis that xylene, as ordinarily obtained, is a mixture of three distinct forms of xylene.

The hypothesis here under consideration is in all essentials typical of most scientific hypotheses in well-developed fields of inquiry. It fulfills all the requirements of a "good" hypothesis mentioned in textbooks of logic, but it also reveals in special clarity two points which are not always stressed in school texts. It reveals (1) that the "verifiable" consequents are logically entailed by the antecedent; and (2) that the hypothesis as a whole is intimately interrelated with a vast body of accepted knowledge, and that it presupposes various (confirmed) hypotheses, principles, and laws.

Turning to the second point first, we find not only that the acknowledged laws and principles of chemistry (atomic theory, theory of valence bonds, etc.) are indispensable presuppositions of the hypothesis, but that scientific procedure itself, as developed in chemistry and as

dependent upon the assumption of a uniformity and causal determination of nature, is part and parcel of the presuppositions. The hypothesis, in brief, is of the same "texture" as the body of knowledge which is chemistry and in which it is to be incorporated. This "texture" of knowledge is of great importance; for even hypotheses that lead to a revision of hitherto accepted laws or theories retain the same "texture" as the laws and theories which they replace.

The first point, however, is equally significant, for the entailment of the consequents is not vouchsafed by a logic based upon "material" or even "strict" implication. "Material" implication, for example, is defined by and depends upon the explicitly stated truth-values of antecedent and consequent; but the corresponding relation, as encountered in "confirmable" hypotheses, must hold antecedently to any definitely assigned truth-values, because the truth-values themselves can be assigned only after the ("confirming" or "disconfirming") evidence has been obtained. If the (testable) consequents are not *entailed* by the hypothesis, they are, strictly speaking, irrelevant, and their truth or falsity can have no bearing upon the truth-value of that hypothesis.

What is true in the case of "material" implication is true whenever implication is extensionally defined. It is true, therefore, in the case of "strict" implication. That certain (testable) "consequents" are consistent with a given hypothesis is not in itself sufficient to guarantee that these "consequents" are at all relevant to the hypothesis in question. The "consequent," "Ice melts at 32°F," for example, is perfectly consistent with the "hypothesis" that "ordinary xylene is a mixture of ortho-, meta-, and para-xylene," but it is hardly relevant; and its confirmation has no bearing whatever upon the truth-value of the latter (cf. Hempel, 1945).

In so far, therefore, as confirmation of a hypothesis depends upon *relevant* evidence, only the logic of entailment is involved; for relevancy is not definable extensionally. It presupposes and depends upon meanings and the interrelations of meanings.

Experimentation

In the preceding section, reference was made to the general "texture" of scientific knowledge, but the term 'texture' was left undefined. We obtain a fairly good "intuitive" idea of what is meant by 'texture' when we compare scientific laws and procedures with statements and arguments in the fields of theology and art, or with the mystical

"knowledge" of primitive man. Through such a comparison we come to realize that scientific knowledge is essentially one of meticulous and testable observations, of quantitative analyses, and of functional laws which integrate experience in conformity with certain broad principles of interpretation. The aspects mentioned are elements in the "texture" of science, and no knowledge can be regarded as scientific if it does not involve at least one of them.

Integrated with the elements just mentioned is the special method of inquiry called *experimentation*. The most exact of the natural sciences would not be what they are without recourse to experimentation, and the less exact sciences are often "less exact" and a matter of "conjecture" only to the extent to which their subject matter precludes experimentation. This is true, for example, in the case of sociology and economics.

The nature of experiments may differ widely in the various sciences, but experimentation as such involves at least two aspects that are common to all experiments. (i) It differs from mere observation because it involves a deliberate disturbance of the "normal" course of events. (ii) It assumes that every event in nature is determined by ascertainable conditions of its physical environment, that a duplication of these conditions entails a duplication of the event, and that changes in these conditions are followed by corresponding changes in the event. The efficacy of (i) evidently depends upon the truth-value of (ii); i.e., it depends upon the broad assumption of "determinacy" in nature.

The purposes for which experiments may be devised are as manifold as are the interests of inquiring minds. At least seven such purposes can readily be discerned.

(1) Experiments may be designed merely to "see what is the case." These are the "exploratory" or "analytic" experiments. Harvey's experiments, for example, which led to the discovery of the circulation of the blood were largely of this type; and so were most of the experiments which Gilbert performed in his study of magnets. The analysis of the facts disclosed in these experiments led both men to "new and unheard of" theories. In general, "exploratory" or "analytic" experiments are encountered in particular where new realms of inquiry are first brought within the range of experimentation. Physiology, for instance, is still largely at this stage of development; and in physiology, "exploratory" experiments are rather common. In order to discover its specific function, it may be necessary, for example, to

suppress an organ by a section or ablation and watch "what happens." In this manner the different functions of nerves—to refer to a specific case—were discovered. The facial (7th cranial) and trigeminal (5th cranial) nerves were cut, and this experiment revealed that section of the facial nerve brings about loss of movement, while section of the trigeminal nerve results in loss of sensation. The respective functions of these nerves, not directly determinable by observation, were thus ascertained by "exploratory" experiments.

(2) Experiments may be devised also as checks on "chance observations." An investigator may happen to notice something unexpected or unusual but may not trust or be satisfied with this "chance observation." He may then resort to experiments in an effort to reproduce or verify what he happened to observe. For example, when Claude Bernard opened the body of a dog that had died of carbon monoxide poisoning, his attention was at once caught by the scarlet coloring of the blood in all blood vessels—in the veins as well as in the arteries, and in the right heart as well as in the left. But was it not possible that the unexpected coloring of the blood was peculiar merely to this particular dog? To check up on his "chance observation" and, if possible, to verify it, Claude Bernard experimented with rabbits, birds, and frogs by poisoning them with carbon monoxide gas and observing the color of their blood. Only when all of these experiments confirmed his first observation in every respect did he proceed with the studies and analyses which in time led to the full explanation of death by carbon monoxide poisoning.

(3) Experiments may be designed, furthermore, to test accepted or "traditional" principles or "laws." Boyle's experiments, as described in *The Sceptical Chymist*, typify this group. In the first part of his essay Boyle describes in considerable detail experiments which were designed to put to a test the "theories of the ancients," notably those of Aristotle and the Peripatetics. The "doctrine of the four elements," in particular, was examined and was disproved by a variety of experiments. Boyle felt—and generations of chemists have been in agreement with him on this point—that, in view of his experiments, "a considering man may very well question the truth of those suppositions which chymists and the *Peripatetics*, without proof, take for granted."

(4) In the fourth place, experiments may be performed in order to re-check generally accepted "facts." Boyle's work again provides numerous examples, for the second part of *The Sceptical Chymist* is devoted to a re-examination of the experiments of his predecessors.

And it is Boyle's contention that through his carefully conducted experiments he has "sufficiently prov'd that those distinct substances, which chymists obtain from mix'd bodies, by their vulgar distillation, are not pure and simple enough, to deserve the name . . . elements."

(5) In the fifth place, experiments may be performed in order to discover laws governing certain events or conditions. Mendel's extensive experiments on the cross-breeding of peas illustrate the type. Over a period of eight years Mendel cross-fertilized his peas and kept minute statistical records of the results. The data thus obtained revealed the relative frequencies of "true forms" and "hybrid forms" which led to the formulation of the well-known "Mendelian" law governing the inheritance of independent (dominant and recessive) characters. The experiments themselves, in their cumulative results, led directly to the law.

(6) Experiments may also be designed to test a newly-formed hypothesis. Galileo's experiments with "falling bodies" illustrate what is meant. A conceptual analysis of uniform and uniformly accelerated motions led Galileo to the conclusion that "the spaces described by a body falling from rest with a uniformly accelerated motion are to each other as the squares of the time-intervals employed in traversing these distances." This "law," obtained as the result of geometrico-conceptual demonstrations, was then "put to the test" in the famous experiments with the incline—experiments which showed that the ratios which "the Author had predicted" were actually the ratios of physical bodies moving with uniform acceleration.

The verification through experiments of the hypothesis (discussed in the preceding section) that "ordinary xylene is a mixture of ortho-, meta-, and para-xylene" illustrates the same type of purpose of an experiment. In each case the hypothesis is conceptually developed prior to the designing and performing of the experiments. In fact, the hypothesis itself is the guiding idea which determines the nature and specific design of the experiments, and is either "confirmed" or "disconfirmed" by the experimental results.

(7) Experiments may be employed, finally, in order to decide between rival hypotheses. One case out of many may illustrate the situation. In ancient and medieval times men generally believed that living things arise spontaneously from dead matter; they accepted, in other words, the hypothesis of "spontaneous generation" or "abiogenesis." Francesco Redi and Abbé Spallanzani, on the other hand, having performed a series of relevant but inconclusive experiments,

denied this hypothesis and maintained instead that there is no life which does not spring from antecedent life; they accepted, in other words, the hypothesis of *omne vivum ex ovo* or strict "biogenesis." Pasteur and Tyndall finally submitted the rival hypotheses to a series of crucial tests. As a result of their experimental findings the hypothesis of "abiogenesis" (spontaneous generation) was discarded and the hypothesis of universal "biogenesis" became generally accepted in the field of biology.

In actual scientific practice experiments designed for the various purposes just mentioned are frequently employed so that they supplement one another, and are interrelated with "theory" and logical analysis to form an "argument" which, because of its variety of aspects and far-reaching ramifications, carries a maximum of weight. Huygens's *Treatise on Light* admirably illustrates what is meant. From the general principle that the causes of "all natural effects" are mechanical motions, Huygens deduced his first premise: that "light consists in the motion of some sort of matter" which exists between us and the luminous body. From the "extreme speed with which light spreads on every side" he inferred that it cannot consist of particles which come to us from the luminous object "in the way in which a shot or an arrow traverses the air." Using an analogy with sound, he reasoned that light, too, might consist of wave motions. This presupposes, however, that light travels with a finite velocity. An analysis of the evidence provided by Roemer gave weight to the assumption and led Huygens to press the analogy further. Recourse to experiments (some of which were performed by Boyle and Torricelli) showed that the medium in which light travels cannot be air. Huygens, therefore, introduced the "ether" as the new medium. Ingenious experiments involving the collision of spheres made of substances of various degrees of hardness then showed (a) that a collision of extremely "hard" particles of "ether" would account for the great speed of light, and (b) that the speed of light cannot be infinite because of the elastic "yielding" and restitution of each particle struck, i.e., because of the very nature of the transmission of the wave motion. The study of colliding particles revealed, furthermore, that light must travel in spherical waves around individual particles as centers; that each particle struck becomes the center of a new spherical wave; that all of these waves are "inconceivably small"; that they move in each direction in such a manner that the unreflected "rays of light" travel in straight lines only; and that rays which intersect or cross do not

destroy or interrupt one another. Upon the evidence thus revealed by experiment *and* analysis Huygens based his famous hypothesis concerning the wave nature of light. From this hypothesis he then deduced certain consequents, showing what is entailed by the wave nature of light with respect to the various phenomena of "reflexion" and "refraction." The entailed consequents Huygens put to numerous experimental tests, the results of which, being in agreement with his predictions, "confirmed" his hypothesis.

Huygens's investigation of the "nature of light" thus reveals in a masterly way the whole process of inquiry from the conception and elaboration of the hypothesis to its ultimate "confirmation"; and it also reveals most pointedly the interdependence of "theory" and experiment.

The development of modern quantum mechanics shows the same interplay of factors in scientific inquiry, but with the additional element of a greater division of labor among many workers in the field. In no case is an experiment an isolated event. Nor is it in itself the sole basis of knowledge. It is one of the means through which knowledge can be obtained, but it is without special value if it is not supplemented by analysis and interpolation, and by the construction of hypotheses.

In their "exploratory" aspects, experiments are devices for discovery. When they are designed to test a hypothesis, they are methods of "confirmation"—although not of "proof." If they are employed as methods of "confirmation," their logical presupposition is the general principle underlying John Stuart Mill's special "canons"—the principle, namely, that "that cannot be the cause of a phenomenon in the absence of which the phenomenon occurs, or in the presence of which the phenomenon fails to occur, or which remains constant when the phenomenon varies, varies when the phenomenon remains constant, or varies in no proportion to the variations of the phenomenon." And this principle itself is, in all essentials, but a restatement of the presupposition of all experimentation stated earlier—the presupposition that every event in nature is determined by ascertainable conditions of its physical environment, and that changes in these conditions entail corresponding changes in the event. Mill's "canons" are specific methodological elaborations of this principle and involve no additional principle of epistemological significance; but the principle, as stated above, entails at least three significant consequences: (1) "causeless" and irrational facts have no place in science; (2) contra-

dictory results of experiments indicate basic differences in the experiments themselves; and (3) the negative results of an experiment are just as much determined by existing conditions as are the positive results. These implications of the principle of determinacy should caution all experimenters not to be hasty in their interpretation of experimental results, and should guide them in the planning of "comparative" experiments as a check on their findings.

In so far as experiments are of an "exploratory" nature the logical context in which they are embedded may be schematically illustrated in the following way: If certain objects (e.g., human beings) are characterized by specific attributes, A and B (e.g., "possessing eyes" and "being able to discriminate between colors"), then we may wonder if certain other objects which are characterized by A ("possessing eyes") are also characterized by B ("being able to discriminate between colors"). What induces us in some particular instance of A to search for B may be the fact that A ("possessing eyes") is an obvious attribute while B ("color discrimination") is not; or it may be the fact that we already know some instances of A (color-blind persons) which are not B. Let us now assume that we have selected a number of objects (e.g., rats) which obviously possess A ("eyes"). The question is, Do they also possess B ("color discrimination")? Only carefully devised and competently performed experiments can provide an answer which is acceptable to science. This answer may be affirmative or negative, but by the very fact that it is an answer to a question it increases our knowledge and constitutes a "discovery."

In order for experimentation to be fruitful and dependable, the experimenter must define his problem with sufficient definiteness to derive from the factual situation which confronts him constant suggestions as to the arrangement and progressive modification of his procedure. Haphazard experimentation seldom leads anywhere. All conditions and factors relevant to the problem under investigation must be carefully noted and must remain under observation throughout the experiment. If the problem is complex, it must be analyzed into its constituent parts, and all subordinate problems which are relevant to the main issue must be investigated. Those conditions and factors which *appear* to be irrelevant must be varied as much as possible in order to make sure that they are *in fact* irrelevant. Care must be taken that no new factors which are relevant are introduced unwittingly. All relevant factors must be varied, if possible, one at a

time in order to determine most accurately what bearing each one has upon the problem in question.

If the problem under investigation cannot be broken down into constituent parts or if it cannot be isolated in specific instances—as is often the case in the biological sciences—"control groups" must be used. The effects of the experimental introduction, elimination, or modification of certain factors in the one group can then be discovered by comparing the individuals of this group with the individuals of the undisturbed "control group." The logical principle behind such procedure is the very same that gives significance to enumerative induction. In neither case are the numbers of instances as such very important. But numbers imply variations; and by comparing a number of instances under control conditions we may be able to eliminate more readily the effects of irrelevant factors.

Experimental procedures are, of course, perfectly reconcilable with the general epistemological point of view defined and defended in this book; for the element of observation present in every experiment is but a renewed appeal to the objects disclosed in first-person experience, and the hypothesis which is to be tested by means of experiments is a presumed "law" of integration bringing order into the manifoldness of first-person experience. The general "determinacy" of events assumed in all experimentation is the basic idea for the construction of the pattern which we call nature, and the "things" with which the scientist deals are what they are only because they are integral parts of that pattern. Our knowledge of these "things" can be obtained in no other way than that described in Chapter III. The physicist, for example, who deals with the behavior of (observed or imagined) "physical systems," *invents* the ideal "states" from which observable properties can be deduced. These "states," formerly regarded as objectively existing "things-in-themselves," are now recognized to be definitional complexities which owe their "existence" to their interrelations with laws and principles in terms of which we integrate the objects of first-person experience. It is immaterial whether we describe these "states" in terms of space co-ordinates and mass points (as did classical mechanics), or in terms of functions involving a suitable number of variables (as does modern quantum mechanics), or in any other way. The important point is that in every case *we* determine how the "state" is to be defined, that the testable consequents of our hypotheses are entailed by our definitions, and that therefore agreement on the definitions constitutes a basis for, and the rationale of, our interpretation of nature.

What is true in the case of physics is true, in principle, in the case of all other sciences as well, and it is true no matter how "realistic" the confessed outlook of the scientists may be. The work of scientists, in the last analysis, aims at a complete integration of experience—an integration which grows out of first-person experience and for which our own minds provide the principles and the key ideas.

More will be said on this point in the next chapter. In the meantime, a more specific problem requires attention.

Operationism

The problem to be discussed in this section can best be introduced by a historical reference. When physiologists discovered that fibrin does not coagulate so long as it remains in the blood vessels of living animals but does coagulate outside the animal body and in dead animals, the "vitalists" maintained that *life* prevents the coagulation in living animals. The opponents of "vitalism," on the other hand (see Claude Bernard), could show (a) that the coagulation of fibrin depends upon certain physico-chemical conditions; (b) that because of certain chemical reactions, these conditions cannot be easily produced but may, nevertheless, occur in living organisms; and (c) that if they occur, fibrin coagulates inside the organism as well as outside. The analysis of the physico-chemical conditions under which coagulation of fibrin occurs was a genuine contribution to scientific knowledge, whereas the vitalistic "explanation" was only an obfuscation of the problem through the use of words.

What happened here in the case of the "vitalists" is a danger which constantly threatens all scientists—the danger, namely, of being misled by suggestive but ill-defined words; the danger of ascribing to words an explanatory significance which they do not have. This danger is especially great in such fields as sociology, psychology, and biology, but is never completely absent in any field of science. References to an "affinity" of chemical substances, for example, may be as misleading as are statements about "vital forces" and "entelechies"; for if the term 'affinity' does not designate merely some particular property of matter, it suggests the presence of occult and mysterious forces which transcend scientific inquiry; and if it designates merely a quality of matter, it seems to be simpler and much safer to avoid the word and to state that under such and such specifically determinable conditions such and such specifically definable substances can be combined.

Are the "forces of dissolution and diffusion" and the various "forces of attraction and repulsion" in physics and chemistry more than words

which seem to "explain" when we are actually ignorant of the real conditions which determine the phenomena? Are "crystallogenic forces," "catalytic forces," and "electro-magnetic forces" *forces* in the same sense? Are they all "forces" in the sense of Newton's second law of motion? Or does our terminology here suggest an identity of kind which does not exist? Consider, for example, the case of catalytic phenomena which depend upon the presence of platinum sponge, and the case of catalytic phenomena which depend upon the presence of concentrated sulphuric acid. Are these phenomena "catalytic" in the same sense? Is it not rather dangerous to speak here about "catalytic phenomena" as such and without reference to the "agents" involved? After all, as an actual chemical process, "catalysis" in the presence of platinum sponge may be quite different from "catalysis" in the presence of concentrated sulphuric acid. Our ignorance of the actual process is not sufficient warranty for speaking of the phenomena as if they were the same in both cases. It is always safer to study the physical circumstances under which certain phenomena occur, to record our findings, and to define the phenomena themselves in relation to the procedures employed in the experiments.

The problem here touched upon was clearly stated and fully discussed in 1866 by the French chemist Deville. In our own times it has found renewed interest in scientific and philosophical circles, and has attained special recognition in the doctrine of "operationism" of which P. W. Bridgman is the principal advocate and expositor. "Operationism," however, has undergone several changes and is at present not a well-defined philosophical position. The "Symposium on Operationism" in the *Psychological Review* of September, 1945, leaves no doubt about this. Bridgman's original contention was that "we mean by any concept nothing more than a set of operations"; that, as a matter of fact, *"the concept is synonymous with the corresponding set of operations"* (*Logic of Modern Physics,* p. 5). In his contribution to the "Symposium," however, Bridgman maintains merely that "for all essential purposes the definition may be specified in terms of the checking operations" (p. 246), and that "operational definitions . . . are in application without significance unless the situations to which they are applied are sufficiently developed so that at least two methods are known of getting to the terminus" (p. 248).

If Bridgman's original statement is taken literally, it entails a complete distortion of the meaning and function of concepts. The meaning of 'length,' to use Bridgman's own example, is not synonymous with

the "physical operations" employed in measuring length. Physical operations of measuring determine only the length of some specific object, not the meaning of 'length' as such. The meaning of 'length,' however, determines the operations of measuring, for without knowing what is meant by 'length' the physicist can make no relevant choice of measuring devices. It simply is not the case that the concept is synonymous with the operations.

Bridgman's later formulations avoid this first difficulty, but they are also less definite and are open to various interpretations. The ambiguity of the situation is increased rather than reduced by corresponding statements of other advocates of "operationism"; for we are told that definitions should be given "in terms" of operations or should be "based upon" operations; that they should "refer to" operations or should "provide specific criteria for the applicability of the concepts." It is not always clear what these statements mean; but it seems that all of them, individually and collectively, amount only to the stipulation that in scientific procedure all concepts employed should be defined in strict conformity with the observed facts or the results of an analysis, and with due consideration for the possibility of experimental checks on these definitions themselves. The vitalistic concept 'vital force' is thus "operationally" meaningless because (a) it is not defined in strict conformity with the results of analysis (for no such "force" can be identified, isolated, or measured), and (b) it is not defined with due consideration for the possibility of experimental checks because, to all intents and purposes, it is non-material and, in the technical sense, non-energetic and is therefore beyond the reach of experimental procedure.

It seems to me that the difficulties and ambiguities of "operationism" spring primarily from a confusion of two distinct situations in scientific procedure—the confusion of "exploratory" experimentation and of concept formation in science. "Exploratory" experiments, as we have seen, are designed to discover "what is the case" or "what happens" when the "normal" course of events is deliberately modified in some specific way. A report of the results of such experimentation is essentially a *description* of the total experimental situation, *not* an explanation of some specific phenomenon. It should therefore include not only a concise statement of the (qualitative and quantitative) results obtained, but also a complete description of the "operations" employed in the experiment, and of the mathematical and logical processes involved in the elaboration or organization of the "data."

Terms which carry a meaning that transcends the purely descriptive purpose of the report are superfluous and misleading and should be avoided in the interest of clarity and faithfulness to the "facts." If the report on "exploratory" experiments is restricted as here suggested, the experiments themselves can be duplicated by other investigators and the results obtained by different investigators can be compared on a purely "factual" basis without the obfuscating influence of ill-defined terms which mean different things to different persons. If this suggestion is followed, innumerable arguments, which *seem* to center around real issues but are actually only arguments about words, will be avoided, and the scientist's time and energy can be devoted to more essential tasks. One instance may suffice as an illustration of what is meant (cf. Feigl). Do "intelligence tests" test *intelligence*? Or is "intelligence" simply that which the tests "define"? The controversy is too well known to require elaboration, but is it a controversy over facts or over words? "Intelligence tests" are obviously designed to find out "what is the case"; they belong, therefore, under the general heading of "exploratory" experiments. But let us discard for a moment the word 'intelligence' and let us ask, What is the factual situation? We then find that different persons taking a certain set of tests answer the test questions with different degrees of "success," some obtaining a "high score" and others a "low score," and still others getting "scores" somewhere between the two extremes. And we discover, furthermore, that there is a fairly high correlation between these test scores and other activities of the persons tested, such as their scholastic standing, their ability to pass other tests, and so on. These facts are beyond dispute; and they are the only *facts* in the case. If the results of testing and correlating the data are always reported in strict observance of the conditions stipulated for "exploratory" experiments, no serious disagreement among investigators can arise. If the correlation between the test scores and other activities of the tested persons is in some cases higher than in others, then this, too, is part of the factual report and, being a statement of fact, is beyond controversy—unless, of course, the charge of "tampering with the facts" is made against some investigator. But even such a charge can be dealt with speedily and effectively by repeating the experiments which presumably yielded the challenged results. All this is simple and clear. But the moment we introduce the term 'intelligence' and insist that our tests are a "measure of intelligence," the trouble begins, for the term 'intelligence' has no precise meaning and is suggestive

rather than clear. It implies a reference to something that transcends the test results as such, and opens the door wide to misunderstandings and difficulties of all sorts. The ensuing controversies, however, are essentially verbal and can be avoided by a strict adherence to the "facts" as disclosed by the tests themselves.

In so far as "operationism" aims at this type of clarification of facts and ideas in science, it deserves our full support, but it is not a new doctrine; and, in the interest of even greater clarity, the term 'operationism' should be avoided altogether, for this term itself, ill-defined as it is, is responsible for much misunderstanding and has often been the cause of heated controversies which, after all, were purely verbal and therefore of no significance.

The situation is quite different when we turn to the problem of "concept formation" in science. Extensive discussions in an earlier chapter showed that the process of defining words for purposes of linguistic communication is one thing, and defining them in the search after truth is something quite different. In the former case, a word is simply a "tag" identifying some referent or group of referents. It is "defined" when we know how to use it correctly. In the pursuit of truth, however, such a definition is inadequate, for it does not integrate the "essential attributes" into a unitary concept which, as the "law" of the thing or event, discloses the nature of the referent.

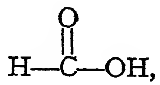
It is possible that Bridgman's reference to "two methods of getting to the terminus" (quoted above) aims at the same distinction, but I am not sure about this. However, be that as it may, if the distinction is kept in mind, we can understand that scientific procedure may lead to increasingly precise definitions of some particular term which was first defined only for purposes of general communication. The maximum of precision is reached when the definitions comprise *all and only* the essential attributes.

Since the essential attributes of a thing or event become known to us only through careful analysis of what experiments disclose and through the use of "confirmable" hypotheses, the definition of concepts is inextricably intertwined with the whole of scientific procedure. The danger lies not in this interrelation with methods of inquiry, but in accepting a mere "tag" for a "concept"; i.e., it lies in being satisfied with a "definition for purposes of communication" or with a "verbal" definition, when what is required is a "definition in the pursuit of truth" or a "real" definition. The former has no special significance in scientific inquiries; the latter is indispensable. The word

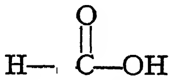
'entelechy,' for example, may be defined for "purposes of communication" so that anyone can use it correctly; i.e., so that anyone can put it correctly into the context of a sentence or/and can employ it in the sense in which it is employed by the "vitalists." But does this mean that the term can now be employed in the service of truth? If so, what are the "essential attributes" of its referent? Hans Driesch, who ought to know whereof he spoke, said that "entelechy" is *not* a constant, *not* energy *nor* energetical intensity, and *not* a force; it *is*—entelechy, "an elementary factor in nature" which "acts teleologically"; it is *not* in space *nor* in time, but acts *into* space and time. Such an enumeration of the attributes which "entelechy" does *not* possess can hardly qualify as a "real" definition. Its negative character contradicts the very first requirement of the "real" definition. The reference to "teleological action," if pressed, turns out to be question-begging; and the contention that "entelechy" only acts *into* space and time puts it beyond the reach of experimental procedure. The whole is a classic example of what a "definition in the service of truth" should *not* be.

A negative illustration, however, does not reveal what a "real" definition is or accomplishes. Let us therefore consider the following case. The term to be defined is 'formic acid.' For purposes of communication it is sufficient to specify that this term (the noun, 'acid,' qualified by the adjective, 'formic,') designates an "acid obtained by the distillation of the bodies of red ants." It is sufficient (although not for all purposes) because it enables us to "use the term correctly." However, if our interest is the attainment of scientific knowledge, we cannot rest satisfied with this definition because it tells us very little about the *essential* attributes of the referent. "Exploratory" experiments reveal that we deal here with a "colorless, mobile, vesicatory liquid of pungent odor" (Webster). In other words, we begin to discover attributes which concern the nature of the substance more directly than does the (incidental) fact that it is found "in the bodies of red ants." Further investigation discloses the fact that the referent in question is a compound of composition CH_2O_2. If we now stipulate that the term, 'formic acid,' designates this compound, we include in our definition some of the truly essential attributes and immeasurably increase the precision of our term. We begin to approximate what a "real" definition should be. Additional experiments show that "formic acid" differs in at least one respect from other acids. It has the characteristics of an aldehyde as well as those of an acid, for it is strongly susceptible to oxidation and shows pronounced bactericidal

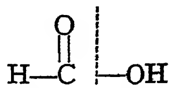
action. This double character is well grounded in the molecular structure of "formic acid," for the structure formula,



reveals the characteristics of both an acid and an aldehyde:



Acid



Aldehyde

Hence, if we now stipulate that the term, 'formic acid,' designates a chemical compound having the molecular structure given above, we no longer deal with a mere "tag" for purposes of identification, but with a conception which, in relation to other laws and principles of chemistry, is the "law" of the compound and its reactions; we have a "real" definition and not merely some stipulation concerning the use of a term (although the latter follows by implication); we have a concept which *explains*, and not a mere word which is misleading or at least not very helpful in its suggestiveness ("obtained by distillation of the bodies of red ants"). And a term so defined is intrinsically defined "in strict conformity with the results of analysis" and "with due consideration for the possibility of experimental checks." There is no evidence that "operationism" adds anything of value to this general practice of defining concepts in the fields of science. No "operations" as such enter into the concept, and the meaning of the concept is most assuredly *not* "synonymous" with the set of operations. The concept is not even defined "in terms of" operations, but in terms of structure. It circumscribes a "construct," and its significance lies in the fact that it provides a "law" governing the (chemical) actions and reactions which depend upon the structure and are entailed by the "construct."

One last point deserves attention before we turn to other matters. Advocates of "operationism" maintain that recourse to "operations" makes the data obtained "public" in the sense of "available for public inspection." Such an assertion is misleading and, in a strict sense, untrue. All knowledge—and scientific knowledge included—has its beginning in the privacy of first-person experience and, in a very real sense, never transcends the sphere of privacy. Knowledge, after all, exists only as it is alive in the minds of men—in your mind and my

mind and in the minds of other persons. But what exists in your mind is privately yours, and what exists in my mind is privately mine, and so with other persons. There is no knowledge as such in books or in journals of learned societies; for what we find on the printed pages are only ink marks which for some person (the writer) were signs signifying ideas he had in his mind when he wrote the report, and which for me (the reader) are signs giving rise in my mind to ideas which I assume to be closely similar to the ideas the writer "had in mind." Communication, as we have seen in an earlier discussion, is not a direct exchange of ideas but an ideational interpretation (on the part of the "receiver") of physical events (sounds or marks on paper) which (to the "sender") are signs for ideas.

But not only does all knowledge have its beginning and its being in the privacy of first-person experience, and not only can it be "shared" only through the two-fold process of communication, but all checks upon that knowledge—observational and experimental—also occur exclusively in the realm of first-person experience. The physical processes of the experiments are, of course, not in themselves the "checks." They become checks only as the results are noted, interpreted, and related to the knowledge to be tested. "Noting," "interpreting," and "relating," however, are processes which go on and can go on only in the privacy of first-person experience. Any reference to an intercourse with other investigators merely begs the question. Specifications of conditions which enable me to repeat your experiment do not make that experiment itself "available for public inspection"; they only enable me to *see for myself* what you have seen *for yourself*. In each case, the observation takes place in the strict privacy of first-person experience. No matter how many spectators observe the "same" event, strictly speaking, each and every one sees the event only within the range of his own experience—and the rest is interpolation. Therefore, when the "operationists" talk about something being "available for public inspection," they either do not know what they are talking about, or their ill-defined phrase refers to nothing more than the fact that in the natural sciences we deal with "things" rather than with mere "objects" of experience (cf. Chapter III); and nobody is going to deny this truism. But in that case "operationism" does not say anything that is particularly new or startling. It has been tradition ever since the beginning of the natural sciences.

So long as we know what we are doing, there is no danger in using the phrase, "available for public inspection"—although it is super-

fluous; but if this phrase is made a premise in an argument defending "behaviorism" or "physicalism," i.e., if it becomes the premise of a metaphysical "ism," it is time to put a stop to the matter and to remember the nature of the integrative process by which alone we come to know that there are "things" which are more than mere "objects" of first-person experience; it is time to remember the fact that the ideational interpolation of first-person experience—no matter how it is carried through in detail—is and remains the ultimate basis of knowledge.

Historical Knowledge

Up to now we have assumed that all sciences integrate experience from one specific point of view, that their interests center around quantitative facts and relations, and that their laws are equations and formulae which, ideally speaking, make possible the prediction of future events. This implicit assumption is warranted in the field of psychology as well as in that of physics, and in sociology no less than in chemistry or in biology. But when we consider history or, rather, the writing of history or historiography, the situation is changed.

The historian is not interested in the purely quantitative point of view nor in the formulation of equations in the sense in which such equations are laws of science. We must, therefore, either broaden our conception of science so as to include the specific point of view of the historian, or we must declare that history is not a science. In either case, our decision will be arbitrary and will probably not satisfy anybody. Controversies on this point are, however, essentially verbal and are not worth the time and effort so often devoted to them. If we broaden the conception of science, we gain little, if anything, of positive value, and succeed only in making the term 'science' ambiguous in meaning. But if we declare that history is not a science, we can allow for the special point of view and the predominant interest of the historian without introducing unnecessary ambiguities. It must be understood, however, that this decision—arbitrary though it is—implies no negative evaluation of the work of the historian. His fervor for truth and his objectivity may well equal the corresponding qualities of any scientist; while his exactitude, the rigor of his criteria, and the meticulous care of his analyses may measure up to the highest standards. But the historian's work differs in ultimate aim as well as in point of view from that of the experimental and social scientists. It is unique and should be considered in its uniqueness.

The aim of the historian is threefold. (1) He hopes to explain *genetically* some particular events of the past. He may try to understand, for example, what caused the first World War or the "War of Independence." He must in that case consider all the factors of geography, ethnology, economics, politics, and so on, which are relevant to the event to be explained, and must integrate them into an organized whole. Or (2) the historian may discuss the significance of an event. What, for example, is the significance of the Russian Revolution or of the Treaty of Versailles? If a question of this type is to be answered, the historian must try to discover to what future events the events considered are relevant, to what future events they contributed or gave rise. Finally, (3), the historian may also attempt to describe the sequence of events covering a more or less well defined "period" of history. He may deal, for instance, with the rise and fall of the Roman Empire, or with the "history" of the Renaissance. If such is his aim, he will probably combine (1) and (2) in manifold ways and weave the results into an all-comprehensive pattern. By themselves, (1) and (2) provide the basis of specialized histories, whereas (3) aims at *Kulturgeschichte*, the history of cultural epochs, the history of culture.

The field of the historian is the whole range of human activities in their social implications and ramifications. Seemingly this is also the field of sociology. But whereas the sociologist is concerned with the repetitive and constant factors or tendencies of human society, the historian is especially interested in the processes and factors which are singular in their space-time occurrence, and which never repeat themselves (cf. Fling, Fortescue). A sociologist, for example, may try to understand the general laws of human conduct involved in the process of "urbanization"; the historian is primarily interested in the specific course which "urbanization" has taken in the United States or in some particular section of the country. That is to say, the sociologist studies the universal aspects of the phenomenon, "urbanization," and attempts to formulate "laws" which govern all such cases. The historian, on the other hand, examines some particular process of "urbanization" as it has actually occurred somewhere, and attempts to describe it as accurately as possible, fusing all relevant matters into a coherent interpretation of this one process. As Fred Morrow Fling put it, the historian is concerned with "tracing the unique life record of humanity," "the unique evolution of man in his activities as a social being."

Of course, historians have discovered certain uniformities in great social movements. They have discovered similarities in the rise of Christianity, Mohammedanism, and Mormonism, and have discovered common elements in the American, French, and Russian revolutions; and, on the basis of his special knowledge, it may happen that a historian may turn sociologist and attempt to formulate the laws of certain "types" of historical events. But what chiefly interests the historian *as historian*, and what justifies history as a special branch of knowledge are the unique aspects or phases of the various movements and revolutions in question. When we study the rise of Mohammedanism "historically," we desire first and foremost to discover how it originated, in what ways it differed from the rise and development of Christianity and other comparable movements, and why; and we try to understand its unique aspects as revealed in its specific space-time setting. We attempt, furthermore, to discern the various factors whose "historic" coincidence assured the success of the movement and whose presence determined the unique course of the development itself. But, as historians, we are not particularly interested in a universal "law of religious movements."

Of course, not every "event" or "occurrence," and not every "action" of human beings is of interest to the historian—no matter how unique it is. According to Rickert, a person becomes a "historical individual" only if he is "the unique bearer of certain values"; i.e., if what he does is uniquely relevant to, and important in, the whole course of societal development. And what is true of persons is equally true of all events or occurrences which are regarded as "historical." They all have some bearing upon "the unique life record of humanity."

The task of the historian is both selective and synthetic. On the one hand, he must gather "evidence" and evaluate his "sources." He must make every effort to discover all pertinent "facts" which are still available and must refuse to be satisfied with merely part of the evidence. On the other hand, he must interpret the facts and must integrate them into one coherent pattern of action and interaction, and must base his "description" of events upon them.

Having discovered new "sources"—monuments, inscriptions, documents, contemporary reports, and so on—the historian must establish their genuineness or authenticity. In this task he may derive aid from such auxiliary sciences as chronology, archeology, epigraphy, paleography, lexicography, and diplomatics. Few "forgeries" survive for any length of time the rigid tests now applied to them. But even

an authentic "document" is not necessarily a trustworthy "source" for the historian. Bias and prejudice may have warped the judgment of its author to such an extent as to result in a wholly distorted picture of the events themselves. The historian, therefore, must resort to various internal and external criticisms of his "sources" and must view them against the general background of facts provided by such specialized sciences as paleontology, archeology, anthropology, ethnology, geography, economics, psychology, and sociology. Often potential "sources" of evidence have been lost or destroyed in the course of time and can never be replaced. The record of the past is therefore always incomplete, and history, as someone has said, is an attempt to find the correct answer to equations which have been half erased.

Once the historian has collected his "facts," he must interpret them and weave them into a coherent pattern. At times the available evidence indicates various possibilities of integration. The writer of history must then formulate the most satisfactory hypothesis to cover all known facts, and in the light of this hypothesis he must re-examine his evidence and look for new "sources." The logic of confirming or confuting his hypothesis is then the same as that of testing a hypothesis in any field of science.

A special difficulty arises for the historian, however, because it is easy to succumb to errors of perspective. We are inclined to judge everybody and everything from our own particular point of view, but persons and events far removed from us in space and time must be evaluated by the standards and conditions of their own culture, not of ours. We cannot write an adequate history of ancient Mexico, for example, in terms of twentieth-century American culture, attributing to the Aztecs our own outlook upon life and our own standards. Historical truth always depends upon the correct "setting" of the events which we describe.

Related to this difficulty is a second, for the historian's selection and evaluation of "data" often depend upon personal convictions and beliefs which unwittingly influence his judgments. It seems inevitable, for example, that the historian of the Catholic Church who ardently believes that there is no salvation outside his Church will interpret the "facts" of the Protestant Reformation in a way which radically differs from the interpretation given by a convinced Protestant. What signifies to the latter a new advance in human freedom, can mean to the former only a regrettable step backwards, depriving millions of

people of their only hope for "life everlasting." Similarly, a person who, consciously or unconsciously, believes in "realism" in art, views "expressionism" and related "isms" as "degenerate art"; while a believer in "modern art" regards "realism" as a failure to comprehend the real nature and purpose of art. If both men should write histories of art, they would in all probability integrate the available facts in different ways and "slant" their histories in conformity with their respective standards of value.

What is true of histories of religious movements and of art is in varying degrees also true of other histories. If the historian is a "materialist," he will stress the importance of economic forces and will "read history" as a struggle between "privileged" and "underprivileged" classes, or as a conflict between the "haves" and the "have-nots" among nations. If he is an "idealist," he will place special emphasis upon "ideological" conflicts. Each point of view tends to affect the "weighing" of evidence and may therefore entail a specifically "slanted" integration of the available "facts."

Such "slanting," however, must not be confused with propaganda or with the deliberate distortion of facts for the purpose of gaining certain ends. The distinction between propaganda and unavoidable "slanting" is, of course, relative, but it is important. The "objectivity" of the writing of history depends upon it.

The ideal of absolute objectivity is probably unattainable in actual practice, because only "selected" facts can be considered by the historian, and "selection" means "evaluation." But if all evaluational presuppositions of a historian are known, we have no difficulty in discerning his particular "slant," or in allowing for it in our understanding of his work. We are then in a position comparable to that in which the physicist finds himself when he realizes that events of the external world may be spatially integrated in terms of either Euclidean or non-Euclidean geometry, but that they cannot be integrated objectively without *some* scheme of order which is logically prior to the integration itself.

Broad perspectives evolved in other fields have often influenced the writing of history. Thus, when Adam Smith published his *Wealth of Nations*, historiography was strongly influenced by his doctrine. Economic motives and the interplay of economic forces became constructive guides for the integration of events. Political ideas—such as those epitomized in the French Revolution—likewise had far-reaching influence upon the interpretation of history. The doctrine of evolution

as well as modern psychology also had their effects. The latter in particular provided new insights into mass-movements, mass-thinking, and mass-emotions, and led to a better understanding of the development and decay of certain states of mind. Recently, historical events have been effectively integrated under the guidance of modern medical knowledge. I have in mind, in particular, MacLaurin's two books, *Post Mortem* and *Mere Mortals,* and Hans Zinsser's well-known work, *Rats, Lice, and History.* Still more recently, geopolitical principles and ideas, too, have dominated historical integrations and evaluations. History, in other words, can be written from many different points of view.

There are special histories of philosophy, science, and technology, of art, literature, and music; of customs, manners, and social institutions; just as there are special histories of political transactions, wars, and successions of governments. But the "new history" aims at an integration of all aspects of human activity and at the conception of an all-comprehensive pattern of cultural development (cf. Barnes, 1925). However, questions pertaining to the ultimate "meaning" of history, the historian *as historian* leaves for the metaphysician; for the answers—be they that history is the "self-revelation of reason," or that it is the "temporal manifestation of freedom," or something else still—far transcend the interests and the methodological point of view of the historian; and the historian, I believe, is right when he regards them as meaningless.

Viewed from the point of view of epistemology, the historian's task —like that of any scientist—is the integration of first-person experience. No event of the past *as past* can be directly observed or analyzed. The "facts" at the disposal of the historian are always and in every case the contents of experience of which he is aware *now.* These contents he integrates, and, subsuming them under the category 'time,' he derives from them his "projection" of relevantly interrelated events —just as we derive our conception of "nature" from a "causal" integration of experience. Actually, the "historical" integration is a necessary supplement to the "causal" integration. It completes the latter and supplies an "explanation" of facts and conditions not otherwise explainable. Historical knowledge is therefore as much the result of imaginative construction as is knowledge in any field of science—but not more so. It is not arbitrary in an absolute sense, but is, and always must be, relative to certain presuppositions of integration.

The historian, of course, is not satisfied with merely knowing *what*

events took place; he desires to discover also *how* and *why* they occurred when and as they did. That is to say, he is also interested in the interdependence of events, and in their significance; he is interested in bringing together those events which are "relevant" to one another and which constitute the coherent pattern of his integration. "Relevancy" is the key term in history (cf. Hempel, 1942).

An event, A, is "relevant" to an event, B, when, and only when, both events are so interrelated that the nature of B cannot be understood without due consideration of the nature of A. The relevancy of A to B, in other words, is grounded in the nature of A and B themselves and is not introduced *ab extra* by the historian. The relevancy of events constitutes, therefore, the basis of historical objectivity and is the ultimate criterion of historical truth.

When a historian resolves a complex event, A, into its relevant sub-events, and when he shows that each sub-event contributes to the nature of A, he gives an "explanation" of A. On the other hand, when he shows to what other events A itself is relevant, he discusses the "significance" of A. "Explanation" and "significance" in history are thus based upon "relevancy" and are subject to objective criteria.

If the relation of relevancy is taken to be a "causal" relation, then, strictly speaking, it should be statable in terms of the same equations which express causal dependencies in the various sciences; for in no other way can a causal connection between events be scientifically formulated. In practice, however, the historian must be satisfied with much less than such a formulation. His explanatory analyses provide at best only generalized sketches of relationships which suggest rather than supply complete explanation.

There are several reasons why this is so. In the first place, historical events are too complex to be readily subsumed under specific equations. It is impossible to obtain sufficient knowledge of all initial and contributory conditions which determine the course of a historical event. Then, too, in many cases the interdependency of events is not in itself clear. Here the historian must depend upon his "insight" or his "constructive imagination." He must "weigh" the evidence and must resort to hypotheses to supplement his "facts."

Moreover, the nature of historiography is such that not every detail which forms part of an event need be considered. How *much* detail is required for a specific historical narrative depends in each case upon the "scale" or the "scope" of the account. What this scale will be for a

given discussion depends, in turn, upon the intention of the historian and is therefore subjectively conditioned.

Lastly, the phenomena which the historian attempts to integrate include facts absent from the world of nature and deliberately excluded in their specificality by students of psychology and sociology—the facts, namely, of individualized human interests and frailties, of individualized aims and motivations. A special bias, a phobia or a mania, a sudden illness or death, the chance meeting of two strangers, a man's or a woman's fancy, the whim of a passing moment—these have at times altered the whole course of history. But the most prominent of all fortuitous factors in history are the "cataclysmic" personalities—the "great individuals" who, because of propitious circumstances, are able to use their special talents and capacities in the furtherance of some "cause" or "ideal" to such an extent that they dominate all else and become the very embodiment of that "cause" or "ideal." The appearance of these individuals the historian accepts as a fact which defies explanation and which nullifies a rigidly causal interpretation of historical events (cf. Hook).

But even if it were possible to explain all historical events causally by subsuming them under the functional equations of science, the specific point of view of history would remain unaffected; for equations "explain" only if they are applied to concretely defined initial conditions, and the course of history would always provide a set of singular and non-repetitive "initial conditions." The uniqueness of events would be unaffected by their explanation in terms of functional equations or of other generalized laws.

Under the heading, "other generalized laws," we may group "dynamic generalizations" of a type which is represented by Thorstein Veblen's "theory of cultural growth" or "theory of cumulative sequence," and of which Pirenne's interpretation of the history of capitalism is a fair example. Every class of capitalists, Pirenne says, is at first animated by a "clearly progressive and innovating spirit." But as time passes and as certain goals are reached, the "innovating spirit" that set those goals declines. The class as a whole becomes conservative. Its activities become regulated by habits, conventions, and statutory law. The descendants of the early capitalists desire to preserve whatever advantages they have inherited. They do not hesitate to place their influence at the service of any government which tries to maintain the *status quo*, and they oppose any movement which tends to alter the established order of things. When such "subversive"

movements arise, the new generation of capitalists demands of the government that it "recognize officially the rank to which they have raised their families" by changing their status from that of a "social group" to that of a legally protected "class" which need not "carry on that commerce which in the beginning made their fortune."

The truth or falsity of Pirenne's generalization is not at issue here. As a "dynamic generalization," it is unquestionably a "theory of progress" formulated in the light of historical observations; and as such a "theory," it is subject to the criteria of truth to which *all* theories are subject. However, for the very reason that it is a "dynamic generalization" or a "law of growth" it is no longer "history." It has become a phase of "social dynamics" with which the sociologist rather than the historian is concerned. Our thesis, therefore, that the writing of history involves a point of view which is, in principle, different from the point of view of the natural and social sciences, finds confirmation whenever we put it to the test.

Perspectives

Much has been said in the preceding sections about the methods and procedures of the empirical sciences. Much more can be said, I am sure, but I believe that the principal topics and problems have been touched upon. In conclusion only one additional point remains to be made. The contention that historiography involves a point of view and an approach to the facts of experience which differ uniquely from the point of view and the approach of the natural and social sciences implies that the methods of the empirical sciences need not necessarily be the only methods through which truth can be established, and that knowledge in the form of laws and systems of laws need not be the only type of knowledge which men may seek.

If we add to the evidence of historiography the evidence available in other fields—such as philosophy, literature, and the arts—in which value judgments and value scales play a predominant role, the conclusion is inescapable that scientific knowledge—important as it is—is only a part of the story and not the whole. Unless we keep this fact constantly in mind, we are in danger of losing our proper perspective with respect to the sciences and of falling victim to an insidious "scientism" which would ultimately destroy the foundations of science itself. The moral and social obligations to which scientists must and do submit transcend the realm of empirical research to find their justification and sanction, not in laboratories and scientific

methods, but in a realm of values—human and humanitarian—which can neither be scientifically analyzed nor reduced to functional equations. Scientists could create the atomic bomb, but political action which is responsive to the moral consciousness of the world must prescribe the future uses of atomic energy. And thus must science become in fact what it has always been in principle: the servant of an enlightened humanity.

CHAPTER VIII

SCIENTIFIC CONCEPTS, LAWS, AND PRINCIPLES

General Characteristics of Scientific Concepts

The initial objects with which scientists are concerned are the objects of "perceptual" experience, the "things" of the world about us—such as planets, plants, and magnets. From these "things" the scientist advances to generalized "inferred" objects—such as electrons, phase waves, and fields of force. As we have seen in Chapter III, both the identification of "perceptual" objects and the interpretation of these objects as "things" require an act of integration which transforms an experienced association of diverse qualities into the complex unity of a "thing." The transformation is accomplished through the employment of concepts which, as specific "laws" of integration, establish the integral unity of "essential" attributes that is indispensable to a full understanding of "things." What is true of concepts in general is especially true of "scientific" concepts. They are the "laws" determining the nature of "things" with which the scientist is concerned. Scientific concepts, therefore, function and have their significance within the framework of the general theory of meaning developed in Chapters I and II.

We have seen in our earlier discussions that language arises whenever a mind or symbol-consciousness takes some specific content of experience (sound, mark on paper, etc.) as designating some other content of experience. As more and more words are coined and, as signs, receive their meaning, the individual meanings come into relation to one another, affect one another, and mutually determine one another's meaning. This mutual determination of meanings is accomplished through the predicative sentence. In it, the subject is related to the predicate and the predicate to the subject, and one is thus determined through the other. Every concept attains its complete meaning only through this (never-ending) process of mutual determination, i.e., through the increasing context in which it is viewed.

Ultimately, therefore, the meaning of a concept is determined by the whole of the language of which it is a constituent part.

Now, because the meaning of a concept depends in such large measure upon the context in which it appears, no meaning is ever rigidly fixed and final. The scientist, however, must insist upon reducing to a minimum the fluctuating penumbra of meaning of all concepts employed in the sciences; otherwise the clarity and exactitude of thought requisite for science could not be achieved. Hence, whenever a new concept is introduced into a field of science, it is at once conceived in the light of all other concepts in that field, and its meaning is fixed with respect to these other concepts as a system of interdependent meanings. Ideally, therefore, every scientist aims at a "closed" system of meanings in his own field—a system of meanings, that is, which depends upon nothing that is extraneous to the field for which it was constructed. So far only the mathematician has actually attained this goal. His system of numbers, as we have seen, is a universal system of signs of order derived by means of a single principle which is universally valid for the system but has no meaning apart from it. The individual numbers have no existence except as members of the derived series. They are what they are only because they are implied by the general rules which define the domain of all numbers, and they depend only upon the relations inherent in the system thus defined.

On the face of it, it seems impossible to duplicate this achievement of mathematics in the empirical sciences, for the empirical manifoldness of experience is not abstractly constructible. The individual perception is always more than a mere "place" in a series of relations and is not reducible to a "where-when" of a space-time order. Actually, however, there exist no fixed and rigid "facts" of experience which are not already permeated with meaning and taken up into some system of concepts. Our previous discussions of the integrative process in experiences have, I believe, established this point. But what is true of the facts of experience in general is especially true of the facts with which the empirical sciences are concerned.

When the physicist deals with sensory phenomena—with colors, sounds, sensations of touch, or anything else—he at once projects them into a new dimension of comprehension and tries to conceive them in terms of a new intellectual standard. He supplements or replaces the qualitative description of the phenomena by quantitative representations. He assigns numbers to the measurable properties of

our sense data, to their varying intensities and their collective aggregates, and he emphasizes those aspects of experience—notably the aspects of space-time coincidences—which can be examined "publicly" or by a community of observers.

Where perceptual objects reveal a manifoldness of gradual transitions and "fluid" contours, the scientist insists upon concepts which provide sharp delimitations, and which eliminate all vagueness of interpretation. Instead of remaining satisfied with the indefinite distinctions of "more" or "less," "nearer" or "farther," "stronger" or "weaker"—so characteristic of perceptual experience—he introduces a scale of numerical values by means of which he succeeds in transforming experienced warmth into the concept of "temperature," kinesthetic sensations into the concepts of "force" and "weight," and, in general, any experienced content into some objectively conceived and quantitatively determinable concept. The concepts of science are, therefore, in a specific sense a step removed from the concepts of perceptual experience. The "data" of science are no longer our perceptions as such, but our perceptions quantitatively fixed and transformed. And it is this quantitative orientation of scientific concepts which imbues the empirical sciences with an exactitude which rivals that of mathematics.

"Things," Constants, and "Constructs"

As far as the scientist is concerned, the "nature" of a thing is determined, not by its sensory qualities, but by its relation to scales of measurement—by its "atomic weight," its "specific temperature," its "electric conductivity," its "index of absorption," and so on. Every physical object, in other words, finds its place within a well-defined scale as such and such empirically determinable "serial value." Copper, iron, and oxygen, for example, are scientifically defined in the table on page 336 (cf. *Handbook of Chemistry and Physics*, 29th edition, 1945).

Other "elements" are defined in corresponding ways. In the case of "things" which are not elements, various features and effects of "compounds" and "aggregates" must also be considered. In each case the real "essence" of a thing lies not in some definable *substance*, but in the relations the thing has to other things, and in the way in which it reacts to these other things; it lies not in the sense qualities disclosed in first-person experience, but in the measurable properties—such as its "exponent of refraction" or its "magnetic susceptibility"

	Copper	Iron	Oxygen
Color	reddish	silver	colorless
Index of refraction	1.39	1.01	gas 1.000272-1.000280
Crystalline form	cubic	cubic	gas or liquid or hexagonal
Atomic weight	63.57	55.84	16.00
Valence	1, 2	2, 3, or 6	2
Specific gravity or density	8.92	7.86	1.429 g/1
Tensile strength lb/in^2	60000-70000	13000-33000	—
Youngs' modulus d/cm^2	$10.19\text{-}12.00 \times 10^{11}$	$18.3\text{-}20.4 \times 10^{11}$	—
Melting point in °C	1083	1535	−218.4
Boiling point in °C	2310	3000	−183.0
Specific heat in cal/g	at 20°C 0.0921	at 20°C 0.107	at −200°C 0.394
Heat of fusion in cal	at 1083°C 42	5.50	at −219°C 3.30
Coefficient of thermal expansion per °C	linear 1409×10^{-6}	linear 9.07×10^{-6}	volume at 100 atm. 486
Thermal conductivity in c.g.s. units	at 18°C 0.918	at 18°C 0.161	at 7°-8°C 0.0000563
Resistivity at 20°C in ohm·cm	1.692	10.0	—

—which place it at a certain point in a scale and which are derived from a quantitative interpolation of observations. These measurable properties are the basic *constants* in terms of which the scientist integrates experience, and the interrelations of which he tries to express in his general laws.

It is well, however, to distinguish between "constants" which, when taken together, define some particular "element" or "thing"—such as the numerical values in the preceding table which, when taken together as given in each column, define copper, iron, and oxygen, respectively —and "*general* constants" which become key concepts for broad fields of science. The following table contains the most important *underived* "general constants" (cf. *Handbook of Chemistry and Physics*, 29th edition, 1945):

Velocity of light	$c = (2.99776 \pm 0.00004) \times 10^{10} \text{cm} \cdot \text{sec}^{-1}$
Gravitation constant	$G = (6.670 \pm 0.005) \times 10^{-8}$ dyne $\cdot \text{cm}^2 \cdot \text{g}^{-2}$
Atomic weight of hydrogen	$H' = 1.00813 \pm 0.00001_7$ (physical scale)
Mechanical equivalent of heat	$J = 4.1855 \pm 0.0004$ abs-joule $\cdot \text{cal}_{15}^{-1}$
Faraday constant (physical)	$F = 96514 \pm 10$ abs-coul $\cdot$ g-equiv^{-1}
Avogadro's number	$N_o = (6.0228_3 \pm 0.0011) \times 10^{23}$ mole^{-1}
Specific electronic charge	$(e/m) = (1.7592 \pm 0.0005) \times 10^7$ abs. e. m.u. $\cdot \text{g}^{-1}$
Rydberg constant for infinite mass	$R_\infty = 109737.303 \pm 0.05 \text{ cm}^{-1}$ (c.g.s. system)

From these *underived* constants several other general constants which play an important role in modern science can be derived. We thus obtain the values of the "electronic charge" [$e = \frac{F}{N_o} = (1.60203_3 \pm 0.00034) \times 10^{-20}$ abs.e.m.u.], of the "mass of an electron" [$m_e = (F/N_o)/(e/m) = (9.1066_0 \pm 0.0032) \times 10^{-28}$ g], of the "mass of a proton" [$M = (H' - E)/N_o = (1.67248_2 \pm 0.00031) \times 10^{-24}$ g], of the "mass of an atom of unit atomic weight" [$M_o = \frac{1}{N_o} = (1.66035 \pm 0.00031) \times 10^{-26}$ g], of "Planck's constant" [$h = \left\{ \frac{2\pi^2 c^3 F^5}{R_\infty N_o^5 (e/m)} \right\}^{\frac{1}{3}} = (6.624_2 \pm 0.002_4) \times 10^{-27}$ erg$\cdot$sec], and so on (*ibid.*).

The discovery of general constants is particularly important because they are the "empirical" core of a science and are necessary to make the equations "work." Furthermore, the general constants, by appearing as invariable factors in different equations, interrelate these equations and thus establish a systemic unity that could not otherwise be achieved. The fact, for example, that the velocity of light in a vacuum is equal to a certain constant, *c*, in Maxwell's equations was sufficient to identify light and electromagnetic phenomena; and the phenomena linked together by Planck's constant, *h*, the basic constant of quantum mechanics, are legion. In all such cases, the numerical interrelations disclosed in the constants suffice to warrant the assertion of a

homogeneity of physical objects which far transcends the heterogeneity of perceptual qualities. Hence, it is not the assumption of metaphysical "essences," but the affirmation of numerical and measurable "constants" which gives to science its power to integrate experience—a power which, in cogency and exactitude, surpasses all other efforts to achieve such integration. The basic "constants" of empirical science provide us with criteria of objectivity for which we look in vain among the qualities of sense perception.

All of this implies a new attitude toward the concepts of science. The development of modern physics has shown irrefutably that the scientist's conceptions of space and time are inseparable from his measurements of space-time magnitudes, and that his knowledge about "atoms" is reducible to knowledge of specific quantitative relations between observed phenomena. All statements about electrons, protons, phase waves, and the like, have meaning only in so far as these "things" of physics are implied by, or definable in terms of, specific quantitative relations of phenomena subsumed under functional laws.

Every new discovery, every new field of investigation, yields additional laws which lead to a greater precision in our definition of basic concepts and which contribute to the increasing concreteness of the "things" of science; for these "things" are determined and defined by the laws of observed phenomena. Time was when the realists in epistemology could argue that their theories were in harmony with the essence and the basic presuppositions of science, but that time has passed. The farther modern physics advances, the more obvious is the fact that the ultimate "things" of this science are ever farther removed, not only from the objects of perception, but from all "pictures" and "models" which we may construct in analogy to the objects of sense perception. In the end there is nothing left for the conception of scientific "things" but their determination through laws.

Is "reality" to be understood, for example, in terms of particles or in terms of waves? All we can gather from the development of physics is that certain experiments lead to the formulation of laws which can be conceived only in terms of "particles," while other experiments lead to laws which can be stated only in terms of "waves." Quantum mechanics merely provides a rule which establishes a "purely symbolic correspondence" (Dirac) between the two types of laws in such a way that only the combination of "particle picture" and "wave picture" yields an adequate description of all aspects of the relevant perceptual phenomena. The basic equations of quantum mechanics completely replace any "realistic" conception of ultimates, and the

concepts of modern physics are introduced only in so far as they are required for, and implied by, the formulation of those equations. Nothing could better illustrate this spirit of modern science than does Pauli's introduction of an elementary particle called "neutrino"—a particle which is electrically neutral, has zero mass, and possesses no describable qualities other than those required to "save" the principles of conservation of energy and momentum (cf. Stern).

If the physicist still speaks of individual electrons, protons, neutrons, or other elementary particles, he does so not because he attributes distinctive individuality to particular "entities," but because in any given situation these "particles" are the focal points of specific and measurable relations. These "entities" are no longer distinct and self-identical "things"—as in classical mechanics—but are "centers of energy." They can no longer be located with exactitude in a space-time scheme, for the conception of an "electron" as a sharply delimitated corpuscle has been replaced by the conception of a somewhat diffused "region of maximum amplitudes." The warranty for the assertion of their "existence" is given, not in direct observation, but in the laws of quantum mechanics which are formulated in an attempt to integrate the contents of perceptual experience.

The real revolution of modern physics, as far as the philosopher is concerned, must thus be seen not in the development of relativity or quantum mechanics as such, but in the changed attitude toward the meaning of scientific concepts—an attitude first revealed by David Hilbert when he defined the basic concepts of geometry in and through the postulates of that science itself, and so made them dependent upon the whole system of relations; and an attitude well justified by the epistemological point of view defined and defended in this book, according to which concepts are not "copies" of things, but "rules" or "laws" of integration. The broad principles of integration themselves determine what the ultimate concepts are to be in terms of which the contents of experience will be integrated. The striving after a "closed system" of scientific terms thus finds its most profound justification.

It may now be pointed out that, as a matter of actual procedure, we define our terms first and then attempt the formulation of laws, so that laws depend upon terms and not terms upon laws. The answer to this objection may be seen in the idea of "successive definition" referred to in the preceding chapter. In the initial stages of a science we may indeed start with "approximate" definitions of certain factors or entities and proceed from such definitions to the first formulations of elementary laws; i.e., we proceed from "tags" to concepts. But once

these (approximative) laws have been stated, they may be employed in the re-definition of the original concepts and so may replace the initial basis from which we started. 'Force,' for example, was originally defined in analogy to the human will, as a power dwelling in physical things. This "primitive" conception led to the definition of 'mass' which, in turn, enabled us to define 'momentum' as the arithmetical product of 'mass' times 'velocity.' Once this equation was obtained, it was possible to re-define the meaning of 'force' in terms of a general law which stipulates that 'force' is equal to the rate of change of momentum:

$$F=\frac{dI}{dt};$$

where $I=$ vector momentum $=f(v)$.

What is true in the specific case of 'force' is, in principle, true in the case of all scientific concepts. No matter how they were first conceived and defined, they can be, and are being, re-defined in terms of general laws; and in this way they fulfill the role assigned to them in the earlier part of this chapter.

One more point must be mentioned at this time. Although the empirical sciences always start from data of observation and return to such data, the "states" designated by the scientific concepts and implied in the various laws are often far removed from such data. "Pointer-readings" and observed "coincidences" are, of course, part of the "facts" of science; but neither the physicist nor the chemist confines himself to such matters. His laws pertain to 'masses,' 'energies,' 'electric charges,' 'fields of force,' 'wave-lengths,' 'phase waves,' 'electrons,' 'photons,' and the like. His laws, in other words, deal with matters which cannot be directly observed and which can be known only as a result of an interpolation of the observable data. Yet the unobserved "states" here referred to are indispensable to a coherent and orderly interpretation of what is observed. As a matter of fact, scientific "explanation" is at heart but a subsumption of the observed phenomena under the laws pertaining to the quantitatively defined "states" designated by such concepts as those just given. All that is required for such an explanation is (a) some rule or set of rules which defines or determines the relation between observed phenomena and the appropriate "states," and (b), some principle or rule which, for the sake of systemic unity, interrelates the different "states" and thus defines a field of science.

The "states" here referred to have often been called "constructs"—and this for good reasons (cf. Margenau, 1935; 1937; 1941). They are not found among the directly observed objects of first-person experience; nor are they necessarily imaginative expansions of such objects. They are defined and delimited in the interest of theories and laws, and the concepts which designate them are deeply grounded in the texture of specific methods and principles.

Time was when the various "states" were so defined that mechanical "models" could show what "really" happened. That is to say, time was when the "constructs" of the exact sciences were patterned after the observable structures of molar mechanics—such as wheels and levers and planetary systems. Although the presumed "states" were not actually observable, it was assumed that *if we could inspect them directly* we would find them to be of the same structure and nature as the "models" devised to represent them. Or if they could not be represented by "models," they might be understood as the ideal "limits" of observed phenomena. The ideas of an 'ideal gas,' a 'rigid body,' and a 'center of gravity' are "constructs" in this sense.

The development of quantum mechanics, however, has revealed an entirely different level of abstraction, for the "constructs" defined by its basic concepts permit no more "visualization" than do the equations in algebra. The "electron," for instance, formerly regarded as a definite particle of specific size and mass, is now defined, not in terms of specified space-time co-ordinates, but in terms of abstract mathematical symbols known as "operators"; and the quantum mechanical description of any "physical system"—such as an "atom," a "molecule," or the like—is given in a "wave function" which defies the construction of a "model." The hydrogen atom is thus defined, quantum mechanically, by the following equation:

$$\Psi_{nlm}\ (\varphi,\theta,\ r)=\sqrt{\left(\frac{2}{na}\right)^{3}\frac{1}{2\pi}\,\frac{2l+1}{2}\,\frac{(l-m)\,!(n-l-1)\,!}{(l+m)\,!2n[(n+l)\,!]^{3}}}$$

$$e^{im\phi}\sin^{m\theta}P_{l}^{m}\ (\cos\theta)\left(\frac{2r}{na}\right)^{l}.e^{-\frac{r}{na}}\,L_{n+l}^{2l+1}\left(\frac{2r}{na}\right).$$

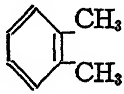
For the most stable or "normal" state of the atom (i.e., for the "K shell," where $n=1$, $l=O$, and $m=O$), this equation, through appropriate substitutions, reduces to

$$\Psi_{100}=\frac{8\pi^{2}\mu e^{3}}{h^{3}}\sqrt{\pi\mu}\ e^{-\frac{4\pi^{2}\mu e^{2}r}{h^{2}}},$$

where μ designates the reduced mass of the atom, **e** (in bold face) is the charge of the electron, h is Planck's constant, r is the distance between the electron and proton, *e* (italics) is the logarithmic base 2.718. . . . , and π has its ordinary meaning. But this is not yet the end.

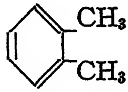
Since, in principle, it is possible to describe atoms by "wave functions," it is also possible to apply this method of interpretation to molecules. When this is done, then, although we obtain a new and powerful instrument for the conceptual integration of experience, we must give up all attempts at "visualization." A simple illustration will make this clear.

Employment of the Kekulé "ring" of benzene in the structural representation of, say, ortho-xylene entails, strictly speaking, two isomeric products, $C_6H_4(CH_3)_2$, namely,



I

and



II

But only one such substance exists. Kekulé himself "solved" the difficulty by suggesting that there occurs a rapid "oscillation" between structures I and II so that ortho-xylene is neither of structure I nor of structure II, but that it has a structure "somewhere between" I and II. The "visualization of the rings," already far removed from actual observation, is thus inadequate for the explanation of the observed phenomena. However, if structures I and II are re-defined in terms of wave functions, it can be shown, on quantum mechanical principles, that the facts of observation are fully accounted for by a new wave function which combines the values of ψ_I and ψ_{II}, representing, respectively, structures I and II. The new equation,

$$\psi_{\text{o-xylene}} = k_I\psi_I + k_{II}\psi_{II},$$

successfully integrates all relevant observations where the Kekulé rings, as "models" of the "states" are inadequate; but we have now no way of "visualizing" $\psi_{\text{o-xylene}}$—nor do we have need of such "visualization."

The "constructs" of quantum mechanics thus take us far beyond the confines of the "sensible" world. But the trend toward an increasingly abstract definition of ultimate "states" is, in principle, discernible in all fields of science, for in science we deal everywhere with concepts

which define "constructed" or "idealized" "states"; i.e., we deal with concepts which, in the interest of precision, go far beyond the intuitive-sensory basis of first-person experience.

The Ideal of an Integrated and "Closed" System

In our discussions of mathematics (Chapter VI), we encountered for the first time in a field of knowledge the ideal of an integrated and "closed" system—the ideal, namely, of a system of concepts and principles which excludes all concepts not definable within the framework and through the principles of the system itself, and of a system which is so well integrated that all laws and relations within it are derivable from the logical presuppositions of that system. A comparable ideal of an integrated and "closed" system is implied in the pursuit of knowledge in the empirical sciences and has already been indicated briefly in the preceding sections. In mathematics the goal of constructing such a system has actually been reached—at least in the domain of numbers. In the fields of the empirical sciences the goal may be unattainable, but it is there as a guidepost and is implied in the whole process of scientific integration of experience. In what follows, an attempt will be made to indicate at least the outlines of such a system in the most exact of the natural sciences, physics, and in particular in the special field of classical mechanics.

All knowledge, so we have seen in Chapter III, arises out of first-person experience. The variegated and multifarious contents of such experience directly or indirectly provide the subject matter of knowledge—scientific as well as non-scientific. Any integrated and "closed" system of scientific concepts, therefore, presupposes and depends upon all of the elements of order involved in that process of integration which leads from the first discernment of "objects" of first-person experience to the assertion that "there exists a world of things"; and it presupposes, in particular, the categories of the "external world" (Chapter III).

Now, among the elements of order which constitute the ultimate basis of knowledge is the category, "quantity." This category especially is a key concept in any field of science, for we know from the history and procedure of science that only that which is itself a quantity or is reducible to quantity is ultimately admitted as an object of science.

Quantity, as we have seen, is either "discrete" or "continuous." If it is discrete, it consists of distinct parts, each of which is in itself

a unique whole, definitely delimited and distinguishable from every other such part. Quantity in this sense is essentially a "multitude." Its measure is "number." The process of its integration is "counting," and the quantity itself, as a multitude, far from existing in nature, is an ideal unity conceived by the apprehending mind and subsumed under the number concept.

If quantity is continuous, it is not made up of distinct parts, but is characterized by a "transitiveness" of relations which transforms the quantity itself into a sequence of "degrees" of "more-less." "Continuous" quantity is a property of qualities of "things" and, as "magnitude," is independent of any creative act of mind. It is "given" with the contents of experience of which it is the measure. "Intensity" and "size" are typical examples of continuous quantity, although they are not the only manifestations of it.

Special importance attaches to the distinction between "permanent" and "successive" quantities; for, if quantity is successive, its forms are "time" and "motion," while, if it is permanent, it is a "spatial extension" and its measure is the "spatial dimension." If extension is considered as one-dimensional; it is a "line" and is measured as "length." If it is considered as two-dimensional, it is a "surface," having length and "breadth." Finally, if extension is considered as three-dimensional, it is a "volume," possessing length, breadth, and "depth." Lines and surfaces have no independent physical existence; they are only "boundaries" of a volume. Volume, however, is the measure of a "body."

Time is experienced as an irreversible sequence of "earlier-later"; but this intuitively felt time, the temporality of first-person experience, is not itself the "time" of physics and chemistry or of any other field of science. In order to become the objective time of science, temporality or *felt* time must be transformed into a *measured* time order. The "given" time-scheme of the immediate contents of experience must be correlated with events which can be employed in the establishing of an objective order of "before" and "after," and of measured "duration." "Clocks" must be constructed, marking off fixed "intervals" of time; and, while measuring time by means of such clocks is necessarily arbitrary and conventional, the intervals so measured provide the only time-scale upon which scientific integration of experience depends. For the scientist, "time" always means "time measured by some clock," and "time order" is the order of events as determined by means of clocks.

What is true in the case of time is equally true in the case of space. We experience objects in "spatial expansion" and "beside" one another, and from these experienced spatial qualities we obtain the concepts of "distance" and "direction." But the experienced "spatiality" of the contents of consciousness is not sufficient for the purposes of scientific integration; it must be transformed into an objective space order. The "space" of the scientist is not the perspective schematism of perceptual experience, nor the "regionally" accentuated "environment" of pre-logical thinking, but an abstract and objectified order of relations based upon measurement. Like objective or measured time, it is a hybrid of concrete measurements and constructive imagination, and—also like time—this "objective" space "exists" only in the mind conceiving it. No wonder, therefore, that Einstein could show that space and time—measured space and measured time, or space and time as the scientist understands them—are so closely interwoven that the relativity of time intervals entails a corresponding relativity of spatial distances. The conditions under which alone the required measurements can be carried out show that the time intervals marked off by "moving clocks" expand in direct relation to the velocity of the clocks, while moving "bodies" contract proportionally in the direction of their motion. Only a unity of space and time, the four-dimensional "space-time continuum," constitutes a stable frame of reference for physical events and provides the specific "co-ordinate system" or scheme of order presupposed in all modern scientific integration of experience.

Within the four-dimensional "frame of reference" the scientist distinguishes between quantities having magnitude only—the so-called "scalar" quantities, such as size and speed; and quantities involving both magnitude and direction—the so-called "vector" quantities, of which velocity is an example.

Once we accept the four-dimensional space-time continuum as our frame of reference, we are in a position to define a "physical body" in the sense in which the scientist thinks of it. In its measurable aspects —and these alone interest the physicist—a "body" is at least a "volume" given at a specified time somewhere in space. That is to say, a "body," regardless of whatever else it may be, is something possessing expansion in the three dimensions of space. Traditionally, such a body has been called "physical" or "material" if it was also "impenetrable"; i.e., if it was of such a nature that no part of it could be made to occupy the same point in space which was occupied at the same time

either by any other part of itself or by a part of some other body, no matter how great a pressure might be exerted upon the body (or the bodies) in question. The discovery of "fields of force," however, has impaired this simple definition, for "fields of forces" frequently interpenetrate, but are, nevertheless, "physical."

If we accept the criterion of impenetrability—and for a vast number of bodies this criterion is sufficient to provide a preliminary definition of their materiality—then we deal with a group of bodies capable of undergoing various "changes." Without recourse to the category, 'change,' we cannot possibly integrate the manifold phenomena which we experience in connection with "physical bodies."

In its broadest sense, 'change' means any alteration or variation whatever. The physicist, however, is particularly interested in changes of position in space during a time interval. Such changes he calls "motion." Now, if motion is viewed as a scalar quantity, we obtain the concept 'speed' (which signifies the time rate at which a body moves through space); and if motion is viewed as a vector quantity, we derive the concept 'velocity' (which designates the time rate of motion in a specified direction). Velocities, of course, may change either in speed or in direction, and the time rate of such change is "acceleration," which is defined by the equation: $a = v_t - v_o$.

Speed and velocity are variables and may become zero. When this happens, motion ceases. The body no longer changes its position in space and is now said to be "at rest." 'Rest,' being thus definable as the "limit of motion" when velocity approaches zero, is derivable from the interrelated concepts already given.

If the acceleration of a body is reduced to zero, the body either continues at rest or in rectilinear uniform motion. "Rectilinear uniform motion" and "rest" are therefore equivalent in a sense which clearly shows the derivative and, therefore, relative meaning of 'rest.'

Accepting the meaning of the terms just defined, we can now redefine the materiality of a body, for it will be observed that "material" bodies—in the sense of our preliminary definition—offer "resistance" to every change in their state of motion or of rest. This "power of resistance" the scientist calls "inertia," and he attributes it to all "material" bodies. Inertia, therefore, in the system of science, is a fundamental property of material bodies; it is, in fact, the criterion of materiality. 'Matter' is now defined as that which possesses inertia; and this definition covers fields of forces as well as "impenetrable" bodies and is given entirely in terms of the concepts and principles of the emerging system of classical mechanics. Whatever extraneous

qualities may have been associated with the concept "impenetrable body" have now been excluded from further consideration.

The measure of inertia is "mass," and 'mass' is one of the most important concepts of physics. It enables us to define 'force' in such a way that all animistic vestiges of pre-scientific thought are excluded from its meaning, and so that it fits smoothly into the system of quantitative concepts which is modern science. For if all material bodies have inertia or mass, i.e., if they all resist every change in their state of motion or rest, then all material bodies, of themselves, remain at rest or in rectilinear uniform motion until acted upon by something external to themselves. All changes in the state of rest or motion of a body must be produced by conditions not inherent in that body. These "conditions" the physicist calls "forces"; and he correlates every well defined change in a state of rest or motion with an equally well defined "force." The measure of such forces is given by the equation: $F = Ma$, where 'M' stands for 'mass,' and 'a' designates 'acceleration'; and only this *measure* of force (not any animistic conception) is significant for the purposes of scientific systematization; i.e., only this measure is essential to the meaning of 'force' in the field of physics.

Employing the concepts so far defined, we can readily derive other concepts by combining the earlier ones in specific functional ways. If this is done, 'momentum' designates the quantity of motion measured by the product of mass times velocity; momentum $= m\underline{v}$; while 'work' means the product of force times the distance through which the body acted upon moves: $W = Fs$.

If we now define 'energy' as that which gives a body the capacity for doing work, we find that this concept, too, is derivable from other concepts which are integral parts of the system. Making the customary distinction between "potential" and "kinetic" energy, we define 'potential energy' as the "energy" resulting from a body's position in relation to other bodies (or from the position of its own parts relative to one another); and the measure of this "energy," which alone appears in the laws of physics, is given by the equation $E_p = mgh$, where 'm' is a mass that has been raised through a distance, 'h,' and is under the gravitational "force," 'g.' 'Kinetic energy' we define similarly through a reference to motion: $E_k = \frac{1}{2}mv^2$. All mystery has thus disappeared from the concept of "energy"; simple equations have taken the place of animistic notions; and concisely and quantitatively defined concepts have become integral parts of the "closed" system of classical mechanics.

If now, in addition to the "principle" of inertia and Newton's "second law of motion" which have been stated above, we introduce several other "principles"—such as Newton's "third law of motion," or the principles of transformation, conservation, and degradation of energy—we have before us most of the basic concepts in terms of which classical mechanics attempted to integrate experience; but we shall break off the detailed discussion at this point. Enough of the "system" of classical mechanics has been presented to show (a) the closely-knit interrelation of the terms, or the integrative character of the "system," and (b) the exclusion from the system of non-quantitative concepts and of concepts not definable in terms of the system itself.

There is, however, another way of looking at this problem. When philosophers refer to "physics," they often forget that the field of physics is loosely defined and that it is subdivided into several specific branches of study which are interrelated but show considerable independence in their development. The unity of the whole field of physics may be seen in the fact that any given kind of energy can be transformed into energy of some other kind; while the diversity of physics arises from the fact that each kind of energy manifests itself in unique ways and necessitates specialized investigations.

The science of mechanics, for instance, deals with the forces of "attraction" and "repulsion," and with the motions induced by the "collision" of bodies. Its subject matter includes "friction," "adhesion," "cohesion," "gravitation," and every form of energy derivable from a body's position in space. It is concerned with every phenomenon which can be subsumed under one form or another of "mechanical" force. The phenomena in question, however, differ basically from the various types of phenomena associated with "heat," "magnetism," "electricity," and "light"—all of which can be integrated only in terms of non-mechanical categories and concepts. Each new type of phenomena, therefore, leads to the development of an additional branch of physics.

The phenomena of "heat," for example, cannot be integrated scientifically until the sensation of "warmth," given in first-person experience, has been objectified and, with the help of measuring devices, transformed into "temperature" or the "condition of a body" which determines the transfer of heat to or from other bodies. The "laws" of thermodynamics presuppose, furthermore, such additional concepts as "pressure" (designating the force exerted on a unit surface), and

"thermal expansion" (meaning the expansion in space of a material body when it is heated); but all of these concepts are of a quantitative nature. They are definable by means of equations and fit smoothly into the general texture of scientific concepts and laws.

The phenomena of "magnetism" and "electricity," involving as they do "directional forces" and "static" as well as "dynamic" conditions, can be integrated only through the employment of additional concepts. Recourse must be had to "fields of forces," "electromagnetic potentials," and other specifically defined "constructs." But whatever the concepts may be that are required for the systemic integration of the electromagnetic phenomena, they are essentially quantitative and statable as functional "laws."

Classical mechanics regards the mass of a body as constant, but modern electromagnetic theories of matter have modified this view. In order to integrate phenomena which transcend classical concepts it is necessary to assume that the mass of an "electron," for example, varies with the speed or velocity of the "particle." If the velocity is small in comparison with the velocity of light, the mass of the "elementary particle" is constant for all practical purposes. But if the velocity exceeds 7,000 miles per second, mass increases rapidly, becoming infinite at the speed of light.

This modification in the conception of mass is one of the far-reaching consequences of Einstein's theory of relativity, and it has found confirmation in the realms of astronomy no less than in the field of electrodynamics. If Einstein's "theory" is accepted—and it has long since become one of the cornerstones of modern physics—the laws of classical mechanics, in so far as they presuppose a constancy of mass, can no longer be regarded as final. They remain valid, however, in the "limiting" cases of low speeds. That is to say, the laws of classical mechanics, based upon the assumption of unchanging masses, turn out to be only first approximations of conditions which are more adequately described by laws that assume a change of mass proportional to any given change in velocity.

The development of modern quantum mechanics has complicated our problem still further, for quantum mechanics provides an integration of all phenomena in terms of concepts and equations based upon the "picture" of wave motions. The last remnants of the old conception of "materiality" have been discarded. Where we used to construct "models" or draw "pictures" of what was supposed to go on within the micro-world of atoms and electrons, we now have left only imagi-

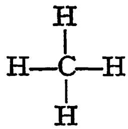
natively created "vehicles for calculation" ("phase waves" and "regions of maximum amplitudes"), the sole purpose of which is to facilitate the formulation of basic equations. 'To explain' no longer means to reveal the "inner workings" of reality, but to subsume observed phenomena under laws (or equations) derived from imaginative "ideal cases" or "constructs." Yet when all is said and done, quantum mechanics and classical mechanics are still of the same general texture—at least to the extent to which the whole domain of physics is concerned with energies and the transformation of energies through motions in space and time. A certain unity is, therefore, still preserved in the midst of all variety and divergence.

When we turn to the field of chemistry we encounter phenomena which, on the face of it, if not in reality, are of a different type. The changes or "reactions" which the chemist studies are not merely changes of position in space but alterations which, in addition, modify the nature or specific characteristics of material bodies. For the chemist, therefore, matter is not simply something which possesses inertia or mass, but something which possesses a number of other properties as well, and which varies in "kind" because of variations in the distribution of its properties. However, the days when the chemist determined the nature of any particular "substance" by the effects it produced upon his senses are over. The purely qualitative distinctions on the basis of sense perceptions are insufficient for the purposes of a science which must insist upon quantitative relations and upon measurement. When "litmus paper" was first used as a test for the acid or alkaline nature of a substance, the way was prepared for an objective basis of chemical knowledge. A means had been found for making the true nature of a substance apparent from its reaction upon other and known substances. A new principle of objectification had been introduced into a field where up to that time no reliable distinction could be made between misconception, self-deception, and fanciful imaginings on the one side and genuine knowledge on the other.

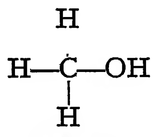
To be sure, in so far as the physicist deals with phenomena other than the purely mechanical relations of masses, he also ascribes diverse properties to material bodies—properties such as density, surface tension, viscosity, specific heat, heat of fusion, heat of vaporization, melting point, boiling point, coefficient of expansion, conductivity of heat, conductivity of electricity, refractive index, and so on. But all of these properties are either reducible to motion or are in some way related to motion. The chemist, however, needs more than this. For him, a

material body is, first of all, a "substance." As a substance, it is homogeneous in composition. If it is a "simple" substance, i.e., if it is chemically irreducible, it is an "element." If it is not simple, it is a "compound." The properties of a compound, however, are not merely the "additive" results of the properties of elements. The chemical compound, in other words, is not a mere aggregate. It is, rather, a structural combination in which new properties "emerge" as the result of an intimate interrelation of the elements. Chemical changes, or "reactions," therefore, by affecting the interrelations of the elements, entail results which are not fully reducible to motion. Such changes are, nevertheless, orderly and subject to law. Their character is in each case determined by the nature of the substances involved. And the nature of these substances is itself such that the concept of each one is, in the sense previously defined, a "law" of that substance, and that all of these concepts are, in principle, interlinked in an integrated and "closed" system. In organic chemistry, for example, two compounds, methane and benzene, provide the key concepts which lead to the systemic integration of practically the whole field; for methane is the key concept in the series of hydrocarbons, and benzene is the key concept in the realm of aromatic compounds. Let me illustrate what I mean.

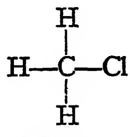
Methane is defined by the structure concept



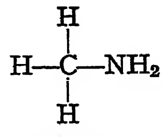
Replacement of one of the hydrogen atoms in this compound by a hydroxyl, halo, or amino (NH_2) group results in compounds which are the initial members of the series of alcohols, halides, and amines, respectively:



Methyl alcohol

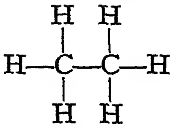


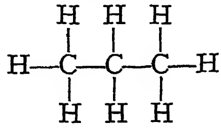
Methyl chloride

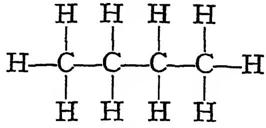


Methyl-amine

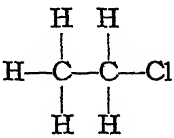
Methane, however, if modified by the addition of one or more carbon atoms, is also the point of departure for a large group of compounds of which ethane, propane, and butane are the first members:

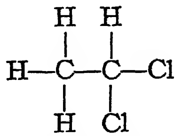

Ethane

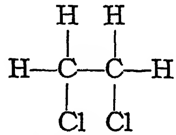

Propane


Butane

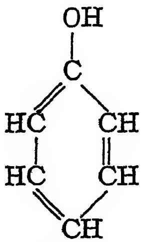
And these compounds, in turn, give rise to specific "derivatives," such as the derivatives of ethane: ethyl chloride; 1,1-dichloroethane; 1,2-dichloroethane, and so on:

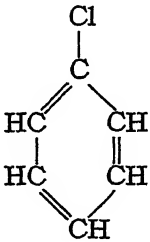

Ethyl chloride

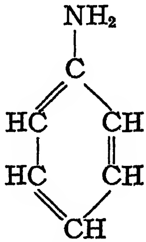

1,1-dichloroethane


1,2-dichloroethane

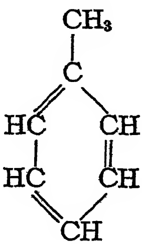
The benzene "ring" is the first link in comparable series. Replacing one of the hydrogen atoms results in compounds such as

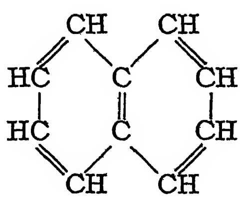

Phenol


Chlorobenzene


Aniline

which are the initial compounds for a great number of "derivatives"; while additions of carbon atoms to the original benzene "ring" leads to the formation of higher aromatic hydrocarbons along two specifically different lines, illustrated by toluene and naphthalene, respectively:


Toluene


Naphthalene

The conceptual integration which is here indicated for the field of organic chemistry is manifested also in the interpretation of chemical changes as "oxidation," "reduction," "hydrolysis," "fermentation," and so on—an interpretation, in other words, which is completely homogeneous with the general texture of chemical terminology, and which avoids all references to non-systemic ideas.

Enough has been said here to indicate the general nature of an integrated and "closed" system within the field of an empirical science. It is not surprising that the character of such a system should be more obvious in physics and chemistry than it is in the social sciences. This difference in "obviousness" is, however, not a difference in principle. The integrated and "closed" system is the ultimate ideal of *all* sciences.

Formalization of the System

The nature of the system as integrated and "closed" (which was made evident in a measure in the preceding section) is particularly manifest in the systemic formalization of the sciences; for such formalizations lead to the construction of deductive systems whose logical rigor meets the most exacting requirements of postulational procedures. That such formalized systems are seldom, if ever, presented in college texts or in books of more general interest is not surprising, because the practicing scientist and the student he trains in his laboratory are concerned primarily with the "laws" that will "work" and yield results, not with the complex set of definitions, assumptions, and postulates which make the systemic deduction of those "laws" possible. But the philosopher, dealing with the nature and structure of scientific knowledge, must go beyond the textbook "summaries" of a science to the system itself, because only a study of that system reveals in proper perspective the various "laws" and "principles."

We shall here consider briefly the systemic formalization of classical mechanics as given by Heinrich Hertz, and that of quantum mechanics as provided by Dirac. It is admitted at once that neither of these formalizations is the only possible integration in its respective field; alternative formalizations are not only conceivable but have actually been developed. Johann von Neumann's formalization of quantum mechanics, for example, attains a logical rigor compared with which even Dirac's admirable achievement is somewhat "loosely knit." But the technical nature of von Neumann's work makes this formalization available only to the sturdiest formalists. The systems of Hertz and Dirac—as here presented—should be regarded only as typical

of the kind of integration found in the empirical sciences, not as the only possible form. They have been chosen because they lend themselves readily to a summary presentation.

The general hypothesis underlying the Hertzian construction is the assumption that it is possible to co-ordinate the observable "masses" of the external world with imaginative "masses" of a type which obeys the laws to be developed as theorems of the system; the assumption, in other words, that the theorems of the system will be "laws of nature" because empirical "masses" are co-ordinated with the ideally conceived "masses" in such a way that the theorems dealing with the interrelations of the latter also describe the interrelations of the former. A corresponding "assumption of applicability" must appear in every formalized system of an empirical science. It is the one assumption which distinguishes an "empirical" system from a purely "formal" system.

With the "assumption of applicability" in mind, Hertz stipulates that three and only three basic meanings must be accepted: space, time, and mass. His "system" consists of the explication of the various relations of space and time—kinematics; of space and mass—statics; and of space, time, and mass—dynamics. Relations involving only time and mass do not occur.

The "space" here assumed as a "primitive idea" is the space of Euclidean geometry with all the characteristics defined by that geometry. "Time," as "primitive idea," is an independently variable magnitude relative to which all changes in experience can be ordered as "simultaneous" or as a sequence of "earlier-later." "Mass" is introduced by definition.

Definition 1: A "mass-point" is a property through which a specific point of space at a given time is univocally correlated with a specific point of space at any other time. It is unchangeable and indestructible.

Definition 2: The number of mass-points in a given space, compared with the number of mass-points in a specified space at a specified time, is the "mass" contained in the given space.

Definition 3: A finite or infinitely small mass conceived in an infinitely small space is a "material point."

Definition 4: A number of material points, conceived as simultaneous, is a "system of material points" or, briefly, a "system." The sum of the masses of the individual points is the mass of the system.

Definition 5: The point of space which is characterized through a

specific mass-point at a specific time is the "position" of that mass-point at that time.

Definition 6: The totality of the positions of all points of a system, conceived as simultaneous, is the "position of the system."

Definition 7: Any possible position of a material point in infinite space is a geometrically "conceivable position."

Definition 8: The totality of the relative positions of the points of a system is the "configuration" of the system.

Definition 9: Every co-ordinate of the system, the value of which cannot be changed without changes in the configuration of the system, is a "configuration-co-ordinate."

Definition 10: Any co-ordinate of a system whose change does not affect the configuration, provided all other co-ordinates of the system remain unchanged, is a co-ordinate of an "absolute position."

There follow definitions which deal with "changes of position in space," with the "magnitudes" and "directions" of such changes, with their relations to a system of co-ordinates, and with the characteristics ("straight," "curved," "shortest," etc.) of the "path" of the changes. Whenever necessary, such definitions are given for "systems" as well as for "mass-points." Additional definitions introduce "degrees of freedom," "planes of positions," "vector magnitudes," and, in general, the mathematical devices requisite to an analytical presentation of the relationships implied in the various definitions. It is not necessary for our purposes to enumerate them all. Nor need we mention the theorems which are entailed by these definitions. We are concerned only with the general characterization of the formalized system.

Definitions of "motion," "velocity," "uniform velocity," "momentum" (as mv), "acceleration," "energy" (as $\frac{1}{2}mv^2$), and the theorems directly deducible from them complete the first part of the system—kinematics.

The second part of the Hertzian system is the most important for our purpose because it is in this part that the systemic integration is really achieved. The definitions and theorems of the first part are here presupposed; but space, time, and mass are now specifically related to the phenomena of the world about us. This is accomplished through the stipulation that henceforth "space," "time," and "mass" must be regarded as "signs for objects of external experience." "Space" is thus the space we measure with a "measuring stick." "Time" is the time we measure by "clocks." And "mass" is the mass we determine by means of "balances."

Given the definitions and theorems of the first part of the system, and given also the stipulation just stated, one and only one "law" need be introduced as a postulate; and from it, in its systemic setting, all other "laws" of classical mechanics can be deduced as "theorems" of the system.

Postulate: Every free system remains in its state of rest or uniform motion in a straightest path.

If a system moves in strict conformity with this postulate, its motion is said to be "natural."

(The similarity between the Hertzian postulate and Galileo's "law of inertia" is striking, but it is not a case of identity.)

A. Motion of free systems:

Theorem 1: A natural motion of a free system is univocally determined by the statement of the position and velocity of the system at a specified time. (Because position and direction of motion determine its path, and the uniformity of motion along this path is determined by the magnitude of the initial velocity.)

Theorem 2: The energy of a free system in motion does not change with time. (Because energy$=\frac{1}{2}mv^2$, where m and v are both constant.)

Theorem 3: A free system moves in such a way that the magnitude of its acceleration is at every moment the smallest which is consistent with the momentary position, the momentary velocity, and the momentary context of the system.

Omitting several theorems pertaining to the path of a free system in motion, we come (in our enumeration) to

Theorem 4: The time integral of energy for the transition of a free system from a given initial position to a sufficiently neighborly end-position is smaller for the natural motion than for every other possible motion which brings the system in the same time from the given initial position into the given end-position.

(This is so because, if the velocity is constant, and the time, T, of transition is given, the time integral of energy, $\frac{1}{2}mS^2/T$, is a minimum.)

This theorem of the Hertzian system corresponds to the Hamiltonian principle of classical mechanics and is of equal significance.

The introduction of the mathematical devices for a differential calculation of natural motions complete this section. We omit these purely mathematical matters and turn to

B. Motions of unfree systems:

Definition: The influence, conceived as independent, which one of two coupled systems exerts upon the other in strict conformity with the basic postulate of motion is called "force."

Implication: For every force there necessarily exists a counter-force.

Theorem 5: Force and counter-force are equal and opposite to each other. (This follows from an analytical representation of "force" as defined above.)

Newton's "third law of motion" is thus a theorem in the Hertzian system.

Additional theorems and definitions pertaining to the "composition" of forces lead to the deduction of Newton's "second law of motion," in the form, $a = \frac{F}{M}$, and to

Theorem 6: The acceleration which several simultaneous forces impart to a system is equal to the sum of the accelerations which the forces would individually impart to that system.

Once these basic "laws" of classical mechanics have been deduced as theorems of the system, the deduction of the more specialized "laws" pertaining to energy, work, equilibria, and simple machines involves no difficulties; but it does not concern us here. The glimpse we have obtained of the Hertzian system must have removed all doubts concerning the contention that, in principle, classical mechanics is an integrated and "closed" system, comparable in its formalization to the postulational systems of the formal sciences, although perhaps not quite so rigorous in its statement.

We turn now to a consideration of Dirac's "system" of quantum mechanics. This seems necessary, not because we need a second example of an integrated and "closed" system in the field of an empirical science, but because (a) an analysis of Dirac's system will show what quantum mechanics is "all about"; (b) it will reveal the specific logical interrelation of classical and quantum mechanics; and (c) it will make evident that the "challenge to causality" is deeply grounded in the principles and theorems of quantum mechanics and is not an *ad hoc* argument based solely upon empirical "evidence." (b) and (c) are indispensable to an understanding of the "meaning" of quantum mechanics and to a philosophical evaluation of its conclusions.

Dirac starts with the definition of the "state of a system" as "a motion that is restricted by as many conditions or data as is possible

without mutual disturbance or contradiction" (second edition, 11). If the "system" under consideration is a photon, its "state" is completely determined by a statement defining its present position in space and its momentum together with a probability statement pertaining to the frequencies with which the photon, in polarization, "jumps" into the "parallel state" or the "perpendicular state" relative to the optic axis. In the case of the photon, the reference to position and momentum expresses the "union of the wave and corpuscular properties of light." The union of wave and corpuscular properties is, however, not restricted to photons. The theoretical work of de Broglie and Schroedinger, and the experimental work of Davisson and Germer, G. P. Thomson, and others, shows that only the union of the particle and wave "picture" provides an adequate interpretation of all phenomena in the micro-world of atoms, protons, neutrons, and electrons.

If, in the field of classical mechanics, we superpose a wave, *a*, upon a wave, *b*, a new wave-state, *c*, which combines in one complex motion the characteristic features of *a* and *b*, is obtained. But what is the result when we superpose the "state of a system," A (as defined above) upon another "state of a system," B? As experiments show, the result is not univocal, but is determined by a probability law pertaining to the relative weights which A and B have in the superposition. Moreover, if an experiment involving an atomic system in a given state is repeated several times under identical conditions, the results may not be the same for each instance of the experiment. But if the experiment is repeated a great many times, each particular result will be observed a definite number of times. The frequency with which it occurs within the total number of observations constitutes a probability ratio which is subject to law.

The task of quantum mechanics is to obtain and formulate the "laws of probability" here referred to.

Now, quantities which, when added, yield new quantities of the same kind can be represented mathematically by "vectors" in a "vector space" of suitably chosen dimensions. It is therefore possible to represent the superposition of "states" in a "vector space." This requires, however, that we represent each "state of a system" by a vector. Let us denote this vector by the symbol, ψ. The simplicity of this symbol does not mean, however, that the vector, when completely analyzed and stated, is a simple matter. The analytic statement of ψ for the hydrogen atom, for example, given in a previous section, should destroy all such delusions. But if ψ symbolizes a certain "state,"

then different "states" may be represented by the same vector symbol with the addition of suffixes, thus: ψ_A, ψ_B, ψ_C, etc. If the superposition of "states" B and C now results in a new "state," A, we can express this fact in an equation:

$$\psi_A = x_B\,\psi_B + x_C\,\psi_C, \tag{1}$$

where x_B and x_C are numbers.

If this equation is accepted, certain facts concerning the nature of the superposition can be deduced: (1) The process of superposition is symmetrical; i.e., the order in which "states" B and C occur is of no significance. (2) All "states" involved in the superposition are *dependent* if the relation

$$x_A\,\psi_A + x_B\,\psi_B + \ldots + x_Z\,\psi_Z = 0 \tag{2}$$

holds. If "states" are not so related they are *independent.*

The maximum number of independent "states" determines the number of dimensions of the vector space.

Assumption: "By superposing a state with itself we cannot form a new state, but only the original state over again" (p. 15). That is to say,

$$x_1\psi + x_2\psi = (x_1 + x_2)\psi,$$

where $x_1 + x_2$ is an arbitrary number other than zero.

Assumption: The coefficients of x can be complex numbers.

The definitions and assumptions so far given imply a fundamental difference between superposition in classical mechanics and superposition in quantum mechanics. (i) In classical mechanics, superposition of a "state" upon itself results in a new "state" with a different magnitude of oscillation. This is not so in quantum mechanics. (ii) In classical mechanics there exists a "state" of zero amplitude, namely, the "state of rest." Such a "state" does not exist in quantum mechanics, for a "zero vector" has no meaning.

The scheme of quantum mechanics outlined by these initial definitions and assumptions becomes a comprehensive physical theory when all the rules governing the mathematical manipulations of the vector magnitudes are given, and when, in addition, rules have been provided which connect the mathematical formalism with the facts of observation.

The rules governing vector analysis need not concern us here; they belong to the domain of mathematics and must find their justification there. But a general characterization of the "vector space" of quantum mechanics will facilitate our understanding of certain aspects of

Dirac's system which are of considerable interest to the philosopher. Two points must be considered.

(i) The geometrical nature of the vector space is such that "conjugate vectors" exist: "Each vector ψ_a in the space of ψ's determines uniquely a vector ϕ_a in the space of ϕ's and vice versa" (p. 22). This symmetry of the ψ's and ϕ's holds for the whole realm of quantum mechanics.

(ii) *Assumption:* The transformation of co-ordinates, $a_1a_2a_3$ . . . and $b_1b_2b_3$ . . . of any two vectors in the passage to some other simple system of co-ordinates takes place in such manner that the number $\bar{a}_1b_1+\bar{a}_2b_2+\bar{a}_3b_3+$ . . . remains invariant. This assumption makes it possible to interpret the ψ's and ϕ's as two different kinds of vectors associated with the same vector space, whereas otherwise they would be vectors in different spaces. It now follows that $\phi_a\psi_b$ and $\phi_b\psi_a$ are conjugate complex, so that $\phi_a\psi_b=\overline{\phi_b\psi_a}$; and $\phi_a\psi_a$ is always real and positive except when ψ_a vanishes.

Through observation, the quantitative values of the dynamic co-ordinates or the momentum of a system are obtained. If these values are called "observables," we can formulate the relation between the mathematical formalism of vector spaces and physical phenomena in the following way.

Basic Assumption: "Each observable is represented in the mathematical formalism by a linear operator that can operate the ψ-vector" (pp. 24-25). That is to say, each observable is represented by an "operator" which transforms the ψ-vector to which it is applied into another ψ-vector whose co-ordinates are linear functions of the co-ordinates of the first.

"Linear operators" can be manipulated mathematically in much the same way in which we add or multiply or form algebraic functions of "observables" in classical mechanics. There is, however, one important difference: *The commutative law of multiplication does not hold for linear operators.* It follows that equations of the form, $(\alpha\beta)\psi_x = \alpha(\beta\psi_x)$, cannot occur in quantum mechanics.

Let us note that as yet the relation between "observables" and "operators" has not been univocally defined. All "observables" can be represented by "operators," but not all "operators" represent "observables." A restriction must therefore be imposed upon the choice of "operators." In Dirac's system (as in quantum mechanics in general) this choice is restricted to the "Hermitian operators." This restriction is accomplished through the following

Assumption: "In the special case when the result of a particular observation made on the system in a particular state is with certainty one particular number, *a* say (instead of being one of two or more numbers according to a probability law), then the Hermitian operator, α, say, representing the observable that is measured and the ψ-vector, ψ_a, say, representing the state are connected by the equation

$$\alpha\,\psi_a = a\,\psi_a.$$

Conversely, if this equation holds, a measurement of the observable represented by α made on the system in the state represented by ψ_a is certain to lead to the result *a*" (p. 30).

This assumption defines the *eigenvalue* which is of crucial significance for quantum mechanics. The assumption entails, furthermore, that every eigenvalue of an observable is a possible result of the measurement of that observable; and that every possible result of such measurement is one of the eigenvalues of the "system." This means that the eigenvalues of a "system" can be calculated and the calculations can be put to an experimental test, thus providing a way of confirming or disconfirming any given interpretation of the "state of a system."

Additional assumptions provide a general method for the "physical interpretation" of the specific mathematical devices employed in the calculations. We omit them here and turn at once to the fundamental problem which now arises.

Because of the "basic assumption" stated above, the "observables" appear in the quantum mechanical scheme as quantities for which the commutative law of multiplication does not hold. This leads to absurdities in the physical interpretations. It is therefore necessary to replace the commutative law by equations which eliminate those absurdities. The new equations are called the *quantum conditions* or "commutability relations." The question is, How can they be obtained?

The "laws" of classical mechanics are valid for dynamical systems that are sufficiently "massive" for the "disturbance through observation" to be negligible. In other words, classical mechanics is adequate for the description of systems consisting of "large numbers" of protons, neutrons, electrons, and the like. Hence, if the "laws" of quantum mechanics, which deal with the component "particles" of the molar systems, can be so formulated that for "large numbers" of such "particles" they yield the "laws" of classical mechanics, the task of quantum mechanics will have been accomplished. In that case, *classical mechanics will retain its validity as the "limiting case" of quantum*

mechanics in the realm of "massive" systems. Conversely, for every important concept of classical mechanics there will then exist a corresponding concept of quantum mechanics, so that quantum mechanics will appear as a specific generalization of classical mechanics. Can this be achieved? In particular, can the "quantum conditions" be formulated in such a way that they appear as a generalization of the classical "law" that all dynamical variables commute?

A mathematical device known as the "Poisson Bracket" plays an important role in classical mechanics, for this "bracket" remains invariant in cases of "contact transformations." Through recourse to Planck's "constant," h, it is possible to formulate quantum mechanical analogues of the "Poisson Bracket" which yield the desired results. These new "brackets" provide the fundamental quantum conditions and reveal where the lack of commutability really lies. In doing this they provide a basis for the commutation of variables not affected by the conditions which prevent commutation in some cases, for they entail the "law" that "dynamic variables referring to different degrees of freedom commute." The new "brackets" also show that classical mechanics is indeed a limiting case of quantum mechanics for all situations in which $\hbar = h/2$ is negligible (h = Planck's constant").

If we select the "phase factors" properly, we can represent the momenta which are conjugate to the generalized co-ordinates, q, by an equation of the form $p_s = i\hbar\pi_s$. Because of the symmetry of the q's and p's in the quantum conditions it is possible to interchange them; and it can be shown that either one of the representatives is determined (apart from numerical coefficients) through the amplitudes of the "Fourier components" of the other. If now a Fourier analysis is made of a ψ-vector whose q-representative consists of what is called a "wave packet," the "packet" represents a "state" for which a measurement of q leads to a result lying somewhere in a "region" of width $\Delta q'$; while a similar measurement of p yields a result lying in a "region" of width $\Delta p'$. The product of these two errors is equal to Planck's "constant":

$$\Delta q' \Delta p' = h. \tag{3}$$

This equation implies that it is impossible to obtain simultaneously a definite value for both q and p. The more accurate the value of one of the variables is, the less accurate is that of the other. When one of them is completely determined, the other is completely undetermined.

Equation (3) is Heisenberg's famous "*Principle of Uncertainty.*" It appears here, not as a generalization from empirical facts, but as a

"theorem" directly deducible from the principles underlying the whole structure of quantum mechanics (and not only the structure of Dirac's system).

The "principle of uncertainty" (which we shall discuss at some length in another section) reveals once more that classical mechanics is a limiting case of quantum mechanics. Classical mechanics assumes that concise numerical values can be assigned simultaneously to all observables. This assumption now turns out to be a valid approximation for all cases in which h, compared with all other factors, is sufficiently small to be negligible.

Our summary of Dirac's system has so far been restricted to stationary "states." The second part of the system includes the relations between "states" at different instants of time, and thus leads to the quantum mechanical "laws" of motion. We shall here refer to only four of the systemic achievements.

(i) Referring back to a "Hermitian operator," Dirac formulates a general "law" for the variation of ψ-vectors with the time; and from this "law" he derives Schroedinger's "wave equation"—one of the most important equations in quantum mechanics. Dirac's system thus accomplishes everything that can be accomplished by the "wave mechanics" of Schroedinger and—because of a mathematical equivalence of the systems—by the "matrix mechanics" of Heisenberg.

(ii) The dynamical variables at a time $t+\delta t$ are connected with the dynamic variables at a time t by an infinitesimal contact transformation. This same condition prevails in classical mechanics. The results of quantum mechanical calculations are therefore formally the same as in classical mechanics.

(iii) Introduction of an angular momentum as a "constant" of the motion is the same as in the classical theory, but it shows here that the idea of an "electron spin"—an idea indispensable to spectral analysis—is reconcilable with Dirac's quantum theory. A relativistic statement of Dirac's "laws" furnishes even some theoretical reasons why the "spin" should be introduced. The idea of the "spin," as may be remembered, is extraneous to and irreconcilable with Schroedinger's "wave theory."

(iv) Observations pertaining to the structure of atoms necessitate the assumption that electrons in an atom move in their own "orbits" so that no two electrons will ever be in the same "orbit." This assumption is Pauli's "Exclusion Principle," about which more will be said later. This "principle" cannot be deduced from the definitions and

assumptions of Dirac's system. It remains a basic assumption even here. But it can be so formulated that it fits into the symbolic scheme of the system as an indispensable supplement.

We have reached the end of our summary of Dirac's integrated and "closed" system of quantum mechanics. This does not mean that we have now before us the final form or the last word in this field of investigation. Dirac himself, in a brief "Conclusion" to his book, has called attention to the unsolved problems and the many difficulties besetting quantum theories in their application to the more complex phenomena of the physical world. But with respect to all such matters we can await the verdict of the physicists. For our purposes it is sufficient that points (b) and (c), mentioned at the beginning of this summary, have become clear to the reader. That is to say, it is sufficient that the reader now has a clearer conception of how quantum mechanics is related to classical mechanics, and that he realizes that the "principle of uncertainty" is an integral part of the system—a "theorem"—and not an *ad hoc* invention.

Additional light has also been cast, I believe, upon the relation between the mathematical formalism of scientific knowledge and the "facts" revealed through observation.

Two other points that may be worth noting can be stated very briefly. The first is that the formalization of an integrated and "closed" system—which is the ultimate desideratum of all science—emphasizes anew the basic difference between "science" and "history" pointed out in the preceding chapter; for no such "system" can ever be the goal of the historian.

The second point is this: Formalization of the "system" in any field of science reveals relationships otherwise easily overlooked or misunderstood. *Laissez faire* economy, for example, rests upon a number of assumptions among which the following two are very prominent: (1) Land, labor, money, and goods are all "commodities" to be sold and bought in a free market. (2) Consumption determines employment. If we examine the Keynesian "system," we find that (2) has been replaced by the (factually more accurate) assumption that consumption and investment together determine employment. In the framework of Keynes's theory this new assumption entails the "theorem" that only under conditions of full employment can there be a consumption of goods so large that it alone suffices to maintain full employment. Actually this situation does not occur, but it may at times be approached. If it should ever actually occur, then the con-

ditions assumed by classical economics would prevail, and the "laws" of classical economics would have full sway. Classical economics, in other words, is a limiting case of Keynesian economics in the very same sense in which classical mechanics is a limiting case of quantum mechanics.

Other economists have challenged assumption (1) of *laissez faire* economics. They deny—and with good reason—that land, labor, and money are "commodities" produced for a market. "Land," for example, is also the habitat of a people, and "labor" is but a collective name for human beings; and both human beings and human habitat have a significance and a worth that cannot be measured in the monetary values of a "self-regulatory" market. The denial of assumption (1), therefore, leads these economists to the development of an economic "system" which is an alternative to, rather than a generalization of, the classical theory—to wit, the economics of a "planned" society. The relative merits of these alternative systems are not now under discussion. The point to be made is simply this, that through the formalization of a system in any field of science the real significance of that system and its relations to other systems become apparent.

Only one question remains: Is it possible to construct one all-comprehensive or *universal* system which includes all fields of science? If such a system comes at all within the reach of our intellectual grasp, it does so, not because we have defined our terms in the "physicalist" manner, but because we have come to know certain broad principles of integration which cut across specialized fields of inquiry. To a consideration of such principles we turn next.

Principles Delimiting the Basis of Science

We consider first and very briefly several principles which define the general basis upon which the integrated systems of the most exact natural sciences have been erected. These principles, together with the "constants" mentioned in an earlier section, provide the ultimate anchorage for the "laws" governing observable phenomena and constitute the set of presuppositions acknowledged by all sciences. Foremost among them, and logically the first premise of all "laws" of science, is the principle of the *conservation of matter-energy.*

Lavoisier discovered experimentally that the quantity of matter is the same at the end as at the beginning of a chemical reaction, and that it can be traced throughout the reaction by its weight. This led to the formulation of the first broad "conservation principle"—the

principle of the conservation of "matter" which, in a form now familiar to every student of chemistry, asserts that matter can be neither created nor destroyed, but only altered in form.

By 1850 it was known that various forms of "energy" could be converted into each other. The conversion of heat into work and of work into heat, in particular, had been experimentally demonstrated. Upon this work and following J. R. Mayer's first formulation, Joule and von Helmholtz established the principle of the conservation of "energy," according to which the total amount of energy in a closed system remains constant, the quantity lost as work reappearing as heat, and the quantity lost as heat reappearing as work.

According to nineteenth-century physics, therefore, neither energy nor matter can be created or destroyed, but only transformed from one form into another. The total amount of both is in the end unchanged. The conjunction of the two conservation principles provides the basis upon which the "mechanists" and "materialists" in philosophy (Moleschott, Buechner, Haeckel, etc.) built their metaphysical systems.

It will be remembered from earlier discussions, however, that for the physicist "matter" is identical with "mass." The principle of the conservation of "matter," therefore, reduces to a principle of the conservation of "mass." But before the nineteenth century was over, J. J. Thomson had shown that there is such a thing as electromagnetic mass and that, theoretically, the mass of an electrified body should vary with its speed. In 1905, Einstein could demonstrate that, as a consequence of the special theory of relativity, "mass" and "energy" must be regarded as equivalent; for it follows from the basic relativity equations that for high velocities the momentum of the moving body increases more rapidly than linearly with the velocity. As the velocity approaches that of light, the momentum becomes infinite.

The equation expressing the equivalence of "mass" and "energy" in relativity mechanics has the form,

$$E = \frac{m}{\sqrt{1-q^2}}. \tag{1}$$

If the body is at rest, $q=0$, and equation (1) becomes

$$E_0 = m, \tag{2}$$

showing that the energy of a body at rest is equal to its mass. But if the body has a velocity approaching that of light, q approaches 1; and if we develop E in powers of q^2, we obtain

$$E = m + \tfrac{1}{2}mq^2 + \tfrac{3}{8}mq^4 + \ldots. \tag{3}$$

The first term of this expansion corresponds to equation (2). The second term represents the kinetic energy of the body according to classical theories (K.E. $=\frac{1}{2}mv^2$). The remaining terms indicate relativity changes with increasing velocity (cf. Einstein). If, in equation (1), we use the second as the unit of time, we obtain

$$E=mc^2 \tag{4}$$

where c is the velocity of light. Solving this equation for actual values shows that if, for example, one kilogram of matter could be entirely converted into energy, we would obtain 25 billion kilowatt hours of energy—a perfectly staggering amount. The success of the atom bomb has provided the most amazing confirmation of these theoretical deductions.

What interests us here, however, is not the practical problem of making these vast stores of energy available for peaceful purposes, but the theoretical implications of the equivalence of mass and energy for the logical structure of science.

The separate principles of conservation of "mass" and "energy" must now be abandoned. In their place we must accept a new principle of the *conservation of "matter-energy."* As matter disappears, an equivalent amount of energy appears, so that the total amount of matter *and* energy remains constant.

It follows from this principle that every change or transformation in the space-time world must be representable as an (individualized) result of the changes in spatial distribution of some constant and self-identical "substratum"—*matter-energy*. The principle of the conservation of "matter-energy," therefore, is the (experimentally well founded) stipulation that in all phenomena of nature something must be regarded as *permanent*; that the "laws of nature" are concerned only with the changing space-time distributions of this "substratum"; and that absolute creation and absolute annihilation are impossible. It is the stipulation of a fundamental unity and homogeneity of the whole of reality, of a unity and homogeneity, that is, which are the logical counterpart of the plurality and diversity of observed and measured phenomena. As theorem or basic postulate (as the case may be), it is an indispensable presupposition of the specialized "laws" of science and is therefore a *sine qua non* of the whole edifice of modern scientific knowledge.

An assertion of unity and homogeneity, no matter how well founded, is not in itself sufficient to account for change in the world about us. The principle of the conservation of "matter-energy," therefore, finds

its logical supplementation in *Carnot's principle,* according to which a phenomenon can occur only where there exists a non-compensated difference in intensity or "tension" of energy, the quantitative value of the phenomenon being always proportional to the difference in intensity of the energies present.

Equalization of the "energy levels" renders energy unavailable for the performance of "work"; and when energy is completely diffused, no physico-chemical process can occur. The index of the relative amount of unavailable energy is called *entropy.* As energy becomes more and more diffused, the entropy of a system increases, the final state of equilibrium being the state of maximum entropy. The *principle of entropy,* as a systemic complement to Carnot's principle, asserts that all processes in a closed system result in an increase in entropy and, more specifically, as "Second Law" of thermodynamics, that all natural processes tend to equalize temperatures throughout the universe, or that heat flows from a place of higher to one of lower temperature, but never in reverse direction. Increase in entropy is thus correlated with the forward "flow" of time; it is a one-directional process. Closed systems and, ultimately, the universe itself have, therefore, a tendency to exhaust their available energy and to "run down."

This "principle of entropy" or of the "degradation of energy systems" can, of course, not be proved by direct experiment. Its essential feature is the assertion that a certain process or change cannot take place; and to demonstrate this requires an "impossibility proof" which is impossible for the universe as a whole. However, the principle is in harmony with known facts in the sense that in all tests conducted under the most rigid conditions no exception to the rule has ever been found, and no contradiction of the principle has been encountered.

What bearing the principle of entropy has upon processes which transcend the purely physico-chemical realm we shall consider in a later section when we deal with its relation to the problems of teleology and purpose as encountered in the field of biology. Here we merely note its systemic significance as a logical prerequisite of the specialized "laws" dealing with changes and transformations of matter-energy in the complex process known as the world about us.

To the three principles just stated there must be added a fourth, *Hamilton's principle,* which asserts that nature, in the changes which it effects, always tends to follow the line of least resistance, i.e., the line which requires the least expenditure of energy. This principle, too, (or its equivalent) is indispensable to any system of "laws" that

integrates physico-chemical processes. Whether it is derived as a "theorem" (as in the Hertzian system) or assumed as an independent postulate is of secondary importance; but no system of "laws" is complete without it. Any mathematical integration of motion or change presupposes it as one of the conditions which make the application of calculus possible.

Principles such as those here enumerated do not pertain directly to objects of observation—as do the specialized "laws"; nor do they perform the specific function of "laws"—although "laws" may at times be elevated to the level of principles. The principles of science here referred to are systemic presuppositions and, in a sense, daring anticipations of "laws." Their worth is established, not by direct experimental evidence, but by their "usefulness" in the organization and integration of broad fields of knowledge. Some of them may have been formulated because of available positive evidence. Others find "confirmation" only in the fact that no evidence is now obtainable which disproves them. But all of these principles define broad outlines of our scientific conception of the world and, together with the "constants," provide the framework within which the special "laws" integrate the facts of observation.

Causality and the Principle of Uncertainty

The "principle of causality" was not mentioned in the preceding section because it occupies a unique position in the system of scientific knowledge and is, strictly speaking, not a presupposition of science in the sense in which the principles referred to above are such presuppositions. The "principle of causality" represents in a very specific sense a general "point of view" from which we integrate the phenomena of experience, rather than a "law" of integration itself. It is not an integral part of the "closed" system which is science, but a stipulation pertaining to the *kind* of "laws" which are parts of that system (cf. Cassirer).

When we examine the "principle of causality" more closely, we are at once struck by the fact that the assertion of "causality," as ordinarily stated, is ambiguous. (i) It may mean simply that "nothing happens without sufficient cause"; or (ii) it may mean "strict predictability." But in either case the assertion of a "causal" interrelation of events is a guiding idea for our integration of experience rather than a "law" in the scientific sense of that term. What is more, the second interpretation is obviously derived from the first by means of

restrictive definitions. Nevertheless, the truth of (i) does not entail the truth of (ii), and the falsity of (ii) does not in itself imply the falsity of (i).

If we accept the "principle of causality" in its first meaning, i.e., if we accept it as asserting that "nothing happens without a sufficient cause," then this is one of the broad assumptions determining the integration of experience. It excludes the "miraculous" from the realm of nature and is an indispensable presupposition of all experiments. Without this presupposition (or its equivalent), all inquiries into the innermost secrets of the material world would end only in blind speculation, mere "hunches" and guesses. Science, as we know it, would be utterly impossible.

Now, the "denial of causality" which stems from the (quantum mechanical) discovery of a "principle of uncertainty" does not affect the essential meaning of this *broad* "principle of causality." This becomes clear, I believe, from the following considerations.

If nothing happens without a sufficient cause, then any event, B, happens only because of its "sufficient cause," A. Conversely—such is the nature of causality in this sense—any "sufficient cause," A, is necessarily followed by its "effect," B. The logical contradiction of this principle is the assertion that A is not followed by B. But this is not the meaning of Heisenberg's "principle of uncertainty"; for the latter implies only that A may be followed with equal probability by either B or C. The "principle of uncertainty," in other words, implies a definite probability ratio of B and C relative to A; the logical contradictory of the broad "principle of causality," on the other hand, admits the possibility of A being followed indiscriminately by either C or D or E or F or any other event which is not B. The contradictory of the "principle of causality" is, therefore, not the (quantum mechanical) "principle of uncertainty," but a "principle of chaos" which implies the possibility that *anything* may happen.

This "principle of chaos," if accepted, reduces to an absurdity not only the functional laws of classical mechanics, but the quantum mechanical attempts at "probability calculation" as well. If, therefore, Heisenberg's "principle of uncertainty" were a true contradiction of the broad "principle of causality," it would eliminate the very foundation requisite to the formulation of *any* kind of "laws" defining interdependencies between events and would thus nullify all efforts directed toward a scientific integration of experience. But this is not what Heisenberg's "principle" accomplishes. The restriction to a finite

number of events which may follow A in conformity with a statable probability ratio preserves in all essentials the meaning of the principle that "nothing happens without sufficient cause." Hence, if it is true that the "principle of uncertainty" entails a denial of "causality," this denial must pertain to a meaning of "causality" other than that which is embodied in the broad statement given above as (i). This "other meaning" is the restricted assertion of "strict predictability" referred to under (ii).

The general ideas of cause and effect have been derived, I believe, from the experience of our own exertions and of the changes produced by them in the world about us. This "animistic" basis of our understanding of causal relations is still discernible in the popular conception of cause and effect sequences. But when the scientists discarded the primitive and animistic conceptions of force and defined 'force' as a "mathematical interpolation between two motions" (Hertz), they also abandoned the old and animistic ideas of causality. They began to express causal relations as functional interdependencies between the numerical measures of variable quantities, and to replace the verbal assertion that a certain event, C, is the "cause" of some other event, E, by functional equations defining the quantitative dependence of E upon C.

The change, although apparently trivial, was fundamental and far-reaching. The verbal assertion that event C "causes" event E implies a distinct reference to time, for it is understood that the "cause" precedes its "effect." In classical mechanics, however, "causal" relations are usually expressed by second-order differential equations which contain no explicit reference to time. The processes so described are all "reversible." The distinction between "cause" and "effect" has been reduced to a distinction between what we regard in each case as the "independent" and what as the "dependent" variable.

However, the solution of these functional equations requires that the "derivatives" constitute in each case a continuous sequence of change, that there be no "leaps" or abrupt turns and reversals; and that the continuity of determination of the dependent variables be preserved. These requirements—which, incidentally, are purely formal requirements grounded in the principles of mathematics—are, when taken together, the equational equivalent of the verbal stipulation that every event have a cause. Conversely, the stipulation that every event have a cause finds adequate expression through the formal requirements of the equations in question.

The functional equations accomplish even more than this. They also provide an absolute solution to a problem which the philosophers discuss only in the most general and ambiguous manner—the problem, namely, of the "uniformity of nature."

As ordinarily understood, the assertion that nature is "uniform" means that the same cause will always produce the same effect. The ambiguity which nullifies the value of this assertion arises from the fact that it is seldom, if ever, possible to obtain exactly the same cause twice. To maintain, for example, that the "cause" which makes a stone drop toward the center of the earth is "the same" as that which keeps the earth in its orbit is only to make a loose statement beset with all sorts of ambiguities. For the physicist, however, no difficulty arises here; for the "uniformity" of nature is adequately represented by the invariable form of his equation, while the observed changes or motions find recognition in the specific numerical values of the variables. It is of course possible that the same quantitative values are never found more than once in nature. The form of the equation which expresses their interdependence remains, nevertheless, the same, thus preserving "uniformity" in the midst of all change. The philosophers must learn to see this "uniformity" not in "things" or "events," but in the relations expressible in functional equations, i.e., in the form of the "laws" in and through which the scientist integrates the data of observation.

Cassirer is right, therefore, when he maintains that the "principle of causality," in so far as it is essential to the exact natural sciences, is nothing but the assertion or stipulation that equations of the type just described are possible, and that every scientific explanation is, in the end, an integration of experience in terms of such equations. This principle, raised to an ideal of all sciences, has dominated nineteenth-century physics; and this principle, I believe, is now challenged by quantum mechanics.

The functional equations of classical mechanics are such that they make "strict prediction" possible. If the "independent" variables of an equation are replaced by concrete values, the "dependent" variables are strictly determined; for they are entailed by the numerical values of the "independent" variables as related in the equation. We can represent this relationship symbolically by writing,

$$V_i \rightarrow V_d,$$

where 'V_i' stands for the "independent" variables, 'V_d' for the "dependent" variables, and '$\rightarrow$' for the relation of entailment.

It is true, however, that any concrete or predictive value of V_d is an entailment of V_i only if concrete and specific values are put in the place of the "independent" variables. That is to say, the *form* of a functional equation is not sufficient to make a precise prediction about some specific V_d; such a prediction requires that we also know with exactitude the specific values of V_i, i.e., it requires that we know with exactitude the *initial conditions* which prevail in the actual situation. Only the *conjunction* of concisely defined "initial conditions" *and* functional "laws" yields the predictive results of which nineteenth-century science was justly proud.

The determination of "initial conditions," being a quantitative determination of "given" factors in some concrete situation, depends upon measurements. Measurements, however (as we have seen in the preceding chapter), cannot always be carried out with complete exactitude. The values accepted as defining the "initial conditions" are therefore, as a rule, not absolute values but idealized "limits" toward which the actual measurements in any given situation tend.

Furthermore, each equation is limited as to the number of variables it contains; that is to say, it is limited as to the number of factors in the "initial conditions" which it includes. Thus, if a bomb is dropped from an airplane in level flight, the range of the bomb, R, is determined by the equation

$$R = v\sqrt{\frac{2h}{g}},$$

where 'v' is the velocity of the plane, 'h' is the height from which the bomb is dropped, and 'g' is the gravitational attraction of the earth. Here R, as "dependent" variable, is determined by the specific values of v, h, and g for each instance of dropping a bomb. But the "range" thus computed for some specific instance can be at best only an approximation to the actual range, for factors other than those designated by v, h, and g may affect the path of the bomb. If in addition to the "independent" variables of the equation we take into consideration also the resistance of the air to the motion of the bomb, and the direction and force of the wind (if there is any), we can approximate more closely the actual range; and by including more and more factors in our "initial conditions" we can attain a precision which approaches certainty. In the "ideal" case, when all factors affecting the path of the bomb have been completely determined, our prediction is completely certain.

It is evident, I believe, that the "strict predictability" implied in the functional equations of classical theories is impeded in at least two respects. Both of these respects pertain to the determination of the "initial conditions" in a "given" or concrete situation.

The difficulties here referred to were, of course, known to the "classical" physicist; but in "classical" physics they were regarded, not as difficulties "in principle" but as "practical" difficulties which in no way impaired the truth or validity of "classical" laws. In "classical" physics, in other words, it was regarded as "ideally possible" to determine the "initial conditions" of an event with such exactitude and precision that absolute certainty of the predictions could be attained. In this sense, therefore, i.e., as an ideal "limit" (attainable *in principle* if not in actuality), "classical" theories assumed the "principle of causality" in the restricted form of "strict predictability." And this "principle of strict predictability" modern quantum mechanics repudiates and abandons.

The conflict between "classical" and quantum mechanical theories is not apparent in the realm of molar mechanics, for the "probability laws" of quantum mechanics (cf. pages 361-362) have been so constructed that for "large numbers" of protons, electrons, and atoms they yield results which are numerically identical with the results obtained by means of "classical" laws. But when we deal with individual protons or individual electrons, the situation is quite different.

In order to predict with precision the position of, say, an electron at a time, t_1, we must know simultaneously and with exactitude the position and velocity of that electron at a time, t_0. But since 'position' and 'velocity' are "conjugate variables," they are so related to each other that it is impossible to obtain simultaneously a definite value for both. The more accurately we determine the value of one, the less accurate is that of the other. The accuracy of the two values taken simultaneously can never exceed the limit of accuracy given by Heisenberg's "principle of uncertainty." This follows (as we have seen in a preceding section) from the basic principles of quantum mechanics, and is also evident from a number of crucial experiments which fully confirm the theoretical deductions.

Whereas "classical" theories assume that we can determine the "initial conditions" of an event with as great an accuracy as we please (and, in principle, with absolute accuracy), quantum mechanics shows that this accuracy can never (not even in principle) exceed a definable limit. Since the "principle of strict predictability" presupposes as an

indispensable condition the (ideal) possibility of absolute accuracy in the statement of "initial conditions," it must be abandoned as soon as this presupposition has been disproved. Quantum mechanics still integrates experience in conformity with "laws," but its "laws" are "probability laws" rather than second-order differential equations. Thus, while the "principle of uncertainty" does not entail the abandonment of "laws" altogether, it necessitates the development of "laws" which differ in form and structure from those "classical" laws which, presumably, make "strict prediction" possible. Only in the realm of gross material bodies do the new "laws" yield the same results as the "classical" laws. In the realm of protons, electrons, and individual atoms the refinements of quantum mechanics entail an integration of experience which gives coherence and continuity to the "pattern of nature" that surpass anything the "classical" theories have to offer. "Classical" laws are thus at best only a first approximation to a systematic integration of "reality" of which the quantum mechanical "laws" are a more adequate expression. And both types of "laws" are a repudiation of the "principle of chaos" which alone is the complete denial of the broad "principle of causality."

In this connection, an additional point may be of interest to the philosopher. It is this: The very "laws" and assumptions of modern quantum mechanics which entail the "principle of uncertainty" entail also the impossibility of giving a concise space-time description of individual and sharply defined "particles" (such as electrons), and entail, therefore, epistemological consequences of great significance (cf. Cassirer).

The "elementary" particles of quantum mechanics are no longer individualized and determinate space-time "entities" to which the laws are "applied" *ab extra*, but (diffused) "states" which are defined in terms of the "laws" themselves. They are not "things" which exist in definitely determinable relations to a space-time frame of reference, but "energy centers" which, together with the "laws" defining their interrelations, give continuity and coherence to the pattern we call "nature." When we speak of the "existence" of electrons, protons, neutrons, and other elementary "particles," or when we assert that these "particles" move and behave in such and such a way, our statements and assertions are warranted only because and in so far as certain "laws" pertaining to spectral lines, cathode rays, trajectories in a cloud chamber, and so on, are valid for the actually observed relations of the phenomena in question. And the reference to "particles"

must always be qualified by the admission that the so-called "particles" also possess the characteristics of "waves." Neither the "particle picture" by itself nor the "wave picture" by itself is adequate for the most comprehensive integration of experience. Only the quantum mechanical correlation of "particle" and "wave"—in the form of somewhat diffused "states" which cannot be concisely defined for all purposes in space-time—is sufficient for that task. And so, at the end of our discussion of quantum mechanics, we return to epistemological considerations which were presented in earlier chapters or sections. The concepts of science do not designate metaphysical "entities"; they are not "tags" identifying "things," but "constructs" making possible that specific kind of integration of first-person experience which we call "scientific." Quantum mechanics, therefore, can be regarded as an especially noteworthy confirmation of the general point of view defined and developed in Chapter III.

The Principle of Relativity

The principles of "causality" and "uncertainty" indicate something about the *type* of equations which, as "laws of physics," serve the purpose of scientific integration of experience. The "principle of relativity," on the other hand, entails the *invariance* of these "laws" with respect to every system of space-time coordinates employed in the process of integration. What this means will become evident when we examine more closely Einstein's theory.

Newton's well-known doctrine of "absolute space" and "absolute time" was generally accepted in "classical" physics as providing the space and time coordinates of physical events. This meant that "space" was regarded as infinite in extent and as Euclidean in character, and that time was conceived as an "equable flux." Both "time" and "space" were regarded as continuous, homogeneous, and unchanging, and as independent of matter. The physicist, however, cannot rest satisfied with abstract definitions. For him, only that which can be measured or reduced to measure is "real." The Euclidean character of space, therefore, interests him only to the extent to which it enters into his measurements of physical events, i.e., it interests him only to the extent to which his measurements of events presuppose it.

Considered in its importance to the physicist, Euclidean space is the space of ideal "rigid bodies." In this space, two points marked on a "rigid body" define an "interval" which remains self-identical and unchanged in length regardless of any linear or angular motions of

the "rigid body." If two such intervals are identical in length in one "place," they are always and everywhere identical in length; i.e., if two ends of an "interval" are so defined relative to the Cartesian coordinates, x_1, x_2, x_3, that the differences of the coordinates, Δx_1, Δx_2, Δx_3, satisfy the equation

$$s^2 = \Delta x_1{}^2 + \Delta x_2{}^2 + \Delta x_3{}^2, \tag{1}$$

then this "sum of squares" will be the same for every position of the "interval" in Euclidean space. Conversely, if this "sum of squares" is the same for every position of the "interval," the space of reference is Euclidean, and the coordinates are Cartesian.

Through the correlation just indicated, Euclidean geometry is transformed from a purely formal science into a basic science of "things." Its presuppositions and assumptions (which, as definitions and postulates, are beyond the question of truth or falsity) are now *hypotheses* in the realm of phenomena and, being hypotheses, may be false. Whether or not they are false for the "physical" space of the external world we shall see in a moment.

The Cartesian systems of coordinates in Euclidean space are such that they transform into each other by linear orthogonal transformations. Since, furthermore, the measurable distance between two points, s, is expressed by equation (1) or, more briefly, by

$$s^2 = \Sigma \Delta x_\nu{}^2,$$

it follows from what has been said above that for two Cartesian systems, $K(x_\nu)$ and $K'(x_\nu)$, the equation,

$$\Sigma \Delta x_\nu{}^2 = \Sigma \Delta x'_\nu{}^2, \tag{2}$$

holds. In other words, $\Sigma \Delta x_\nu{}^2$ is an "invariant" with respect to the transformations permissible in Euclidean space. Stated more generally, this means that in a space of Euclidean character only such, and all such, quantities as remain invariant despite all linear orthogonal transformations, have objective or "physical" significance. The system of equations which is "classical" physics is thus valid for every system of Cartesian coordinates in the (Euclidean) space of reference.

It is clear, I believe, that *if "physical" space is Euclidean*, then the laws of "classical" physics are sufficient for the integration of all relevant phenomena. The question is, of course, Is the assumption of Euclidicity of "physical" space warranted by the facts? More specifically, are there "spaces of reference" in motion relative to each other which are "physically" equivalent in the sense indicated by equation (2)?

To these questions "classical" mechanics gave an unequivocal and affirmative answer. Experiments carried out in laboratories which are stationary upon our earth reveal nothing of the great speed with which the earth itself moves around the sun. The "laws" are invariant despite the earth's motion. This invariance is not found, however, if the "laws" are computed under conditions of arbitrary motion of the reference system. The "law" of falling bodies, for example, is the same whether it is calculated relative to the surface of the earth or relative to the car of a "roller coaster" which is stationary on the earth; but it is by no means invariant if it is calculated relative to the car of a "roller coaster" as that car plunges down or shoots up the steep inclines of its tracks. Thus, as far as "classical" mechanics is concerned, there appear to exist equivalent spaces of reference—the so-called "inertial systems"; and relative to these systems the "laws" of mechanics can be stated in the simplest form possible.

Putting it differently, if K is an inertial system, then every other system, K′, which is at rest or in rectilinear uniform motion relative to it is an equivalent inertial system, and the "laws of nature" are invariant for all such systems. This statement is in all essentials Einstein's "principle of special or restricted relativity."

Let us now assume that in an inertial system, K, an event, E, is determined relative to the Cartesian coordinates, x_ν, and relative to the time, t. How can we calculate the coordinates, x'_ν, and the time, t′, of E relative to an inertial system, K′, which is in rectilinear uniform motion relative to K? In "classical" mechanics the solution of this problem is made possible by two assumptions—the assumptions, namely, that (1) time is absolute (i.e., that the time, t′, of E relative to K′ is the same as the time, t, of the same E relative to K), and that (2) length is absolute (i.e., that the length, s, of an "interval" which is at rest in K is the same as the length, s′, of that "interval" in K′). The transition from K to K′ is made in each case by means of the "Galilean transformation equations":

$$x' = x - vt$$
$$t' = t$$

Under these conditions, two events, one in K′, the other in K, are "simultaneous," if the following equation is satisfied:

$$x'_\nu{}^{(1)} - x'_\nu{}^{(2)} = x_\nu{}^{(1)} - x_\nu{}^{(2)}. \tag{3}$$

The question is, Are the assumptions which underlie this equation —the assumptions, namely, of "absolute time" and "absolute length" —warranted by the facts?

The principles and laws of "classical" *mechanics* are in full accord with the assumptions, but the Maxwell-Lorentz equations dealing with electromagnetic phenomena are not. The Maxwell-Lorentz equations, however—and they alone—explain satisfactorily the propagation and aberration of light in moving bodies, and various phenomena observed in double stars. These equations are therefore indispensable to modern electromagnetic theories. Hence, if a decision must be made between retaining the Maxwell-Lorentz equations or retaining the "classical" assumptions of "absolute time" and "absolute length," the physicist does not hesitate. That this decision *must* be made is evident from the following considerations.

The Maxwell-Lorentz equations entail as a consequent that in a vacuum light travels with a constant velocity, c, with respect to a definitely defined inertial system, K. In view of the "principle of special relativity," this consequent must be true also for every other inertial system, K′, which is in uniform rectilinear motion with respect to K. But it follows from the "Galilean transformation equations" that a ray of light which moves with a velocity, c, relative to K, moves with a different velocity, c', relative to K′—where c' depends upon the direction of motion of K′ with respect to K, being greater than c when K′ moves toward K, and being less than c when K′ moves away from K. The space of reference defined by K (e.g., the quiescent ether of "classical" theories) is thus uniquely distinguished from all other reference spaces which are in rectilinear uniform motion relative to it. But if this is true, it entails the denial of the "principle of special or restricted relativity." All experiments show, however, that there is no "preferred" reference system for the velocity of light; that the velocity of light is constant for *all* reference systems; and that, therefore, the "principle of special relativity" is valid. Since only the two assumptions stated above entail this contradiction between "facts" and "principle," the assumptions become untenable and must go. But what is to take their place? After all, some stipulations concerning space and time are indispensable for the laws of physics.

We must recognize and take seriously the fact that only *measured* "time" and *measured* "length" are of concern to the physicist; that only the "time" indicated by "clocks" and the "length" determined by means of some "measuring device" enter into his equations. Let us therefore view the matter from this angle.

Let us assume that we have a "clock," U, which is at rest relative to K, and a "clock," U′, which is at a great distance, $r_{UU'}$, from U,

but also at rest relative to K. In order to adjust U′ so that it runs synchronously with U, we send out a light signal from U at a time, t_U. This light travels through a vacuum until it reaches U′. At the moment when the light signal arrives at U′, this "clock" is set to indicate the time,

$$t_{U'} = t_U + \frac{r_{UU'}}{c},$$

where c is the velocity of light. If an additional signal is sent from U a "second" later, it will arrive at U′ a "second" after the first signal, thus making possible a perfect adjustment of U′ relative to U.

So far we have assumed that both "clocks," U and U′, are at rest relative to K. How will the adjustment be made if U′ belongs to a system K′ which is in rectilinear uniform motion with respect to K? If we accept the "principle of special relativity" and also the "principle of the constancy of the velocity of light," we encounter no difficulty (cf. Chapter VII of *A Philosophy of Science*). But let me state this in a different way.

The constancy of the velocity of light with respect to K is expressed by the equation,

$$\Sigma(\Delta x_\nu)^2 - c^2 \Delta t^2 = 0, \tag{4}$$

where Δx_ν represents the differences in the coordinates resulting from the propagation of light, and c is the velocity of light. A corresponding equation,

$$\Sigma(\Delta x'_\nu)^2 - c^2 \Delta t'^2 = 0, \tag{5}$$

expresses the constancy of the velocity of light relative to K′. Now, if U′ in K′ is to be adjusted relative to U in K, equations (4) and (5) must be mutually consistent with respect to the transformation equations which lead from K to K′. This condition is fulfilled when we accept the "Lorentz transformation equations" (rather than the "Galilean" set):

$$x'_1 = \frac{x_1 - vt}{\sqrt{1 - \frac{v^2}{c^2}}} \tag{6}$$

$$t' = \frac{t - \frac{v}{c^2} x_1}{\sqrt{1 - \frac{v^2}{c^2}}} \tag{7}$$

where c is the velocity of light, and v the velocity of K′ relative to K.

In these transformation equations, "time" and "length" are so interrelated that both become dependent upon the velocity of K′. The faster K′ moves relative to K, the longer will be a "time interval" transferred from K to K′, and the shorter will be a "space interval" or a "length" transferred from K to K′.

This interdependent *relativity* of time and space "intervals" is often considered the most important result of Einstein's "special theory of relativity" when actually it is only a means to an end—the end being the preservation of the *invariance* of the "laws" of physics with respect to any inertial systems, K and K′, which are in rectilinear uniform motion relative to each other.

We have previously seen—equations (1) and (2)—what "invariance" means in "classical" physics. We must now recognize that equations (1) and (2) do not define an "invariance" with respect to all the Lorentz transformations, for the new "invariance" is given by the equation,

$$s^2 = \Delta x_1{}^2 + \Delta x_2{}^2 + \Delta x_3{}^2 - \Delta t^2, \tag{8}$$

where "time" enters as a "fourth dimension" into our calculations. According to (1), i.e., according to Euclidean geometry, s^2 is necessarily positive, and vanishes only when the two points in question coincide. But according to (8), the vanishing of s^2 is the invariant condition under which alone the two space-time points can be connected by a light signal. That is to say, s^2, as determined by equation (8), is an "invariant" with respect to all transformations involving the "Lorentz transformation equations." The "laws" of physics are thus valid only for the systems of coordinates which satisfy the "principle of special or restricted relativity."

There is another way of looking at this. The physicist is concerned primarily not with the space-time coordinates of an "event," but with the "event" itself. He desires to formulate his "laws" describing that "event" in such a way that they are valid regardless of the specific system of coordinates employed at a given time. His problem is solved by the "special" theory of relativity for all those systems of coordinates which are at rest or in rectilinear uniform motion relative to one another. If the "Lorentz transformation equations" are employed, i.e., if the "laws" of an "event" are so formulated that they are consistent with the "Lorentz equations," then these "laws" are "invariant" or valid for all systems of coordinates which are connected by the "Lorentz transformations." That this "invariance" of the "laws" entails appropriate "contractions" or

"expansions" in the four-dimensional "continuum" which serves as "reference space" is of secondary importance. Philosophers would not have emphasized it so much had they always kept in mind the fact that our conceptions of "physical" space and "physical" time are but interpolations in terms of measurements of the experienced "spatiality" and "temporality" of first-person experience. Newton's

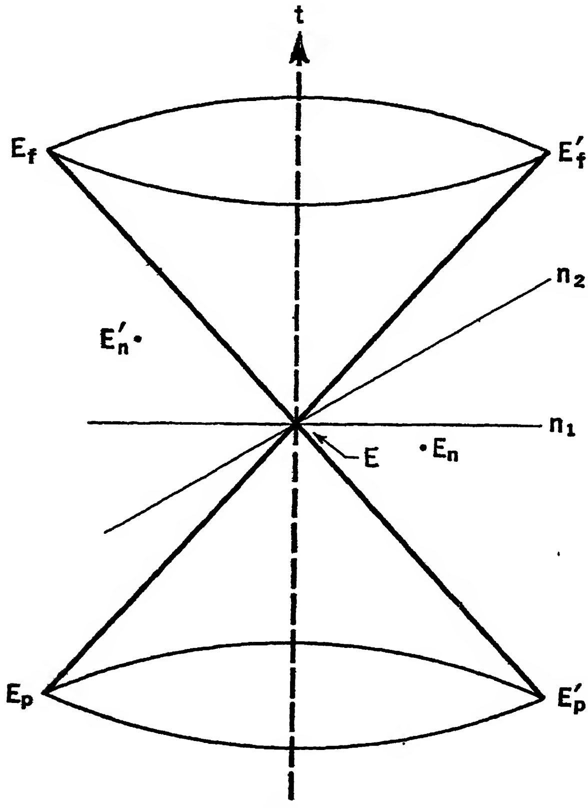


Fig. 4

ill-founded doctrine of "absolute space" and "absolute time" has led to the view that "space" and "time," as far as science is concerned, are metaphysical "entities" existing in immutable self-sufficiency and unaffected by "events." This doctrine has been exploded by Einstein's theory; for relativity physics entails the contention that the "events" alone are "real," and that "space" and "time" are important only as a "frame of reference" for the "laws" describing the "events."

Three additional points are of interest: (i) It is obvious from equations (6) and (7) that for all cases in which v is negligible compared with c the "Lorentz transformation equations" are identical with the corresponding "Galilean" set. It is obvious, in other words, that Einstein's "special" theory of relativity, far from invalidating "classical" physics, yields the "classical" laws for the "limiting" condition of K′ moving with a negligible rectilinear uniform motion relative to K. "Relativity" physics may therefore be regarded as a *generalization* of "classical" physics.

(ii) The relativity of "time" entailed by the "special" theory permits an absolute determination of "past" and "future" with respect to any given "now" but leaves undetermined the "simultaneity" of events which are widely separated in space. Let the "arrow," t (Fig. 4), represent the "flux" of time in the forward direction. Let E be an "event" at a given moment in time, "Now." Let E_p and E'_p be "events" which precede E in time in such a way that they can just be connected with E by a light signal; and let E_f and E'_f be "events" which follow E in time in such a way that E can just be connected with them by a light signal. Let the lines, $E_pEE'_f$ and E'_pEE_f represent the light signals. It then follows that all "events," E^v_p, which lie inside the "time cone," $E_pEE'_p$ can be connected by light signals with E, and that they are therefore prior in time to E. The "time cone," $E_pEE'_p$, in other words, represents "absolute past" with respect to E. Similarly, all "events," E^v_f, which lie in the "time cone," $E_fEE'_f$, are later in time than is E, and the "time cone," $E_fEE'_f$, thus represents "absolute future" with respect to E. But the regions defined by E_pEE_f and $E'_pEE'_f$, respectively, are in this sense neither "past" nor "future" with respect to E, for "events" such as E_n and E'_n cannot be connected by light signals with E. They are so situated that no physical means at our disposal enable us to determine whether they precede E, follow E, or are simultaneous with E. Whether the line marked n_1 or the line marked n_2 or any other similarly drawn line through E represents the "events" which we regard as "simultaneous" with E depends therefore upon our choice of a reference system. The relativity of "time intervals" thus entails the relativity of "simultaneity." "Events" which are simultaneous with respect to a reference system, K, are not "simultaneous" with respect to a system, K′, which is in motion relative to K (cf. *A Philosophy of Science*, Chapter VII).

(iii) If the vectors of momentum and energy are so formulated that they satisfy the "Lorentz transformation equations," it can be

shown that, as a consequence of the "special" theory of relativity, the momentum increases more rapidly than linearly with the velocity. It becomes infinite as the velocity approaches that of light. The same equations show that the energy of a body at rest is equal to its mass. They show, in other words, that the "principle of the equivalence of matter and energy" is an entailment of the "special" theory of relativity (cf. the preceding section).

So far we have considered only the "special" or "restricted" theory which assumes that "all *inertial* systems are equivalent for the description of physical phenomena," and we have pointed out that the "laws" of physics can be so formulated as to remain "invariant" with respect to these systems. It is characteristic of "inertial" systems, however, that they are either at rest or in rectilinear uniform motion relative to one another. If systems of reference move relative to one another with other than rectilinear uniform motions, they are not "inertial" systems, and their non-uniform or rotary motions seem to affect the "course of events" in such a manner that, with respect to them, the "laws" of physics can no longer be regarded as "invariant." Is there any way in which this difficulty can be overcome and the "laws" can be made "invariant" with respect to *all* systems of reference and irrespective of their motions? To this question Einstein's "general" theory of relativity gives an affirmative answer.

It is a well-known fact of "classical" physics that the ratio of the masses of two bodies can be defined in two fundamentally different ways—(i) as the reciprocal ratio of the accelerations imparted to the bodies by the same force ("inertial" mass), and (ii) as the ratio of the forces acting upon the bodies in a gravitational field ("gravitational" mass); and it is an equally well known fact that the masses so defined are equal. This equality of "inertial" and "gravitational" masses is not only fully confirmed by experiment but follows directly from Newton's "law" governing the motion of a body in a gravitational field—the "law," according to which ("Inertial" Mass) × (Acceleration) = (Intensity of the Gravitational Field) × ("Gravitational" Mass). If this "law" is true, then acceleration is independent of the nature of the body only when "inertial" mass and "gravitational" mass are numerically equal.

Let us now assume that K is an inertial system, and that certain masses are without acceleration relative to K. If K′ is a reference system which is in uniformly accelerated motion with respect to K, then the masses just referred to have an equal and parallel acceleration relative to K′. But if K′ is accepted as the reference system and

therefore as unaccelerated, the masses behave with respect to it as if they were attracted by a gravitational field. (For an elementary demonstration see Chapter VII of *A Philosophy of Science.*) System K′, if assumed to be "at rest" and if supplemented by an appropriately chosen "gravitational field," is thus equivalent as a frame of reference to the "inertial" system, K, where no "gravitational field" is present.

This "principle of the equivalence" of "inertial" and "gravitational" systems—intimately connected with the equality of "inertial" and "gravitational" masses as it is—entails the possibility of extending the theory of relativity to all types of reference systems, for it implies that the same masses may be considered as determined either by inertia alone (system K) or by inertia in conjunction with gravitation (system K′).

For "small regions" systems of co-ordinates may be chosen in such a manner that relative to them material bodies move freely and without acceleration. Such systems are in all essentials equivalent to the "inertial" system of the "special" theory. This theory is therefore contained in the "general" theory as a "limiting" case. But if we now extend the principle of relativity so as to include systems of reference which are in "rotary" motion relative to the "inertial" systems, we are forced to conclusions which are irreconcilable with the conception of a Euclidean character of space.

Let us suppose that in a system K′, which is at rest relative to an "inertial" system K, there is given a large (stationary) disk. Let us suppose, furthermore, that we have placed "measuring rods" along the periphery of this disk, P, as well as along the diameter, D. We then have

$$\frac{P}{D}=\pi,$$

which is the Euclidean ratio.

Let us now assume that K′ and the disk rotate around an axis perpendicular to the plane of the disk and relative to K, and let us determine the "length" of the "measuring rods" with respect to K. We then find that the "measuring rods" placed along the diameter of the disk remain unchanged in "length," but that the "measuring rods" placed along the periphery have undergone a "Lorentz contraction" (in conformity with the laws of the "special" theory of relativity), That is to say,

$$\frac{P}{D}>\pi,$$

where the divergence from π is proportional to the angular velocity

of the disk. Thus, the laws of the geometrical configuration of "rigid" bodies can no longer hold for "rotating" systems or for systems involving "gravitational" fields.

Parallel considerations pertaining to measured time "intervals" reveal corresponding distortions, because it follows from the "Lorentz equations" that "clocks" placed on the periphery of the rotating disk go slower than do "clocks" placed at the center where they remain "stationary."

Generalization of the "principle of relativity" leads thus inevitably to the conclusion that gravitational fields determine the metrical laws of the space-time continuum relative to which we interpret any given "event." More specifically, wherever there are gravitational fields, there space is of a non-Euclidean character.

The question is, Can the "laws" of physics be so formulated that they remain "invariant" with respect to every conceivable system of reference and irrespective of the non-Euclidean character of space? This problem, of course, is largely one of finding adequate mathematical forms for the necessary equations, and we shall omit all discussion of the tensor equations, first developed by Riemann and later modified and supplemented by Levi-Civita and Weyl, which make possible a "generalization" of Euclidean space so that the latter retains its significance for the "limiting" conditions of infinitesimal regions and the absence of gravitational fields. Suffice it to say that such a "generalization" has actually been achieved.

It is one thing, however, to "generalize" geometrical space in a purely formal way, and it is another thing to correlate this "generalized" space with "gravitational fields" so that in the absence of "gravitational fields" the character of space is Euclidean, while in the presence of "gravitational fields" the deviation of space from the Euclidean ratio is proportional to the strength of the "fields." Einstein succeeded in doing the latter, and he did so by re-stating the "laws" governing "gravitational fields."

According to the "laws" of "classical" physics, all "gravitational fields" are determined by the density of ponderable matter. We know, however, that the "special" theory of relativity entails the essential equivalence of matter and energy, and that it thus leads to a replacement of the "scalar density of matter" by a tensor equation including electromagnetic energy as well as ponderable matter. Matter itself, in other words, becomes the principal part of the "*electromagnetic* field." The mathematical expression stating this fact is the "energy tensor of

matter." But if matter is to be linked up with gravitation, there is need also for a tensor denoting the energy density of the "*gravitational* field." The formulation of this latter tensor Einstein has achieved, using Poisson's differential equation of "classical" physics as his "model." The result is a new basic "law,"

$$R_{\mu\nu}-\tfrac{1}{2}g_{\mu\nu}R=-KT_{\mu\nu}, \qquad (9)$$

Einstein's "field equation," which entails the energy principle of matter and leads to the Newtonian equations under certain specified "limiting" conditions.

This "law" is valid for *all* systems of coordinates, irrespective of their relative motions, provided only that the respective "gravitational fields" are taken into consideration. It enables the physicist to derive the equations of the "special" theory as well as the "laws" of Newtonian mechanics from a single integrating principle. It entails the relativity of "time intervals" and of measured "distances" no less than the observed "bending" of light rays in "gravitational fields" and the advancement of the perihelion of Mercury. For certain "limiting" conditions equation (9) yields the values of the "Galilean transformation equations" and of Euclidean geometry and thus provides the reference system of "classical" physics. Applied to the universe as a whole, Einstein's "law" leads to results which are in satisfactory agreement with the facts underlying Hubble's "law" of an "expanding universe."

It is unnecessary here to discuss in detail these consequences of Einstein's "general law"; our interest centers exclusively on one point: the "integrative" power of the new "law." And this power is truly amazing. The most diverse facts of the broad field of physics can be subsumed under this one "law." The integration of experience is now possible irrespective of any particular system of reference we may select; for every system of reference with its specific "field of gravitation" has been shown to be equivalent to every other system of reference with its "gravitational field"; and relative to all these systems the "laws of nature," if properly formulated, are "invariant." The systemic unity of these laws and their universal scope have thus been assured beyond all question. Contradictions which formerly arose between the fields of "classical" mechanics and "classical" electrodynamics have been eliminated and the general field of physics has been unified beyond anything possible in pre-relativity days.

However, even the "general" theory of relativity as now formulated may some day be superseded by a still more "general" theory, for the

present formulation includes the "gravitational field" and the "electromagnetic field" as logically distinct structures. A "unified" field theory which eliminates this distinction would therefore achieve still greater formal coherence, but all attempts (including Einstein's own) to develop such a "generalized field theory" have hitherto led to unsatisfactory results. "Unified field theories" can be constructed in a purely formal way (Weyl, Kaluza), but their formalism does not provide a real solution of the basic problem.

The one approach which promises success in solving the problem of "unifying" relativity principles and quantum mechanical ideas, but which is not a generalized field theory, seems to be Eddington's posthumously published *Fundamental Theory*, in which the conditions of observability laid down by the relativity theory and the quantum theory, respectively, are combined into a new and comprehensive principle satisfying the distinctly different needs of both theories. The new principle takes into account the requirement of the relativity theory that a coordinate, relative to which an observed velocity can alone be determined, must itself be observable; and it specifies that a coordinate is observable only if it is a relative coordinate of two entities both of which have uncertainty of position and momentum in the geometrical frame (the quantum mechanical condition of observability).

Admittedly this principle introduces an uncertainty into the frame of reference. But since this uncertainty is restricted by Heisenberg's "principle of uncertainty," it is no absolute barrier to the formulation of laws governing the "field" determined by a system of such coordinates.

The highly technical and intricate development of Eddington's theory need not concern us here. It cannot be presented without recourse to complicated mathematical proofs and deductions. The reader is therefore urged to study Eddington's own presentation of the new ideas.

Pauli's "Exclusion Principle" and the Principle of Quantum Mechanical Resonance

The "principle of causality" and the "principle of uncertainty" characterize the *type* of equations which are acceptable as "laws" of nature. The "principle of relativity" provides the possibility of formulating these equations in such a way that they remain *invariant* with respect to *any* system of coordinates we may select.[1] The three prin-

[1] This is true at least *in principle*—although it has not yet been possible to state all equations of quantum mechanics in relativistic form.

ciples together determine the nature and form of the "laws" of contemporary physics. We turn now to a consideration of principles which transcend the field of physics and integrate physics and chemistry in one broad system of interrelated laws.

The history of chemistry demonstrates in a most remarkable manner how progress in a field of science is intimately bound up with a gradual transition from a qualitative interpretation of experience to a quantitative and mathematical analysis.

When the chemical elements were first arranged in the "Periodic Table," their qualitative differences and "family resemblances" became associated with definite "place numbers." "Empty" places in the "Table" made possible the prediction that the "missing" elements, when discovered, would possess such and such specific qualities. In case after case the prediction was fulfilled, thus proving that the "atomic number" signifies something fundamental in the constitution of the elements, not merely an accidental place in the "Table."

After the electron had been discovered, Moseley could show (1913) that the "atomic number" is an index of the number of electrons surrounding the nuclei of the atoms in question. It now became possible to give a quantitative explanation of the varying lengths of the "periods" in the "Periodic Table," for it could be shown (Bohr, 1913) that the "family resemblances" of the elements are correlated with the distribution of the atomic electrons in definite orbital patterns of 2, 8, 8, 18, 18, and 32 elements. In the "Bohr atom" these patterns are correlated with the principal or total "quantum number," n, which designates all the transitions from one "orbit" into another which are possible for an electron within a given atom. n is always a whole number.

Sommerfeld and W. Wilson, working independently, could then show (1915) that a second or subordinate "quantum number," l, is needed if we are to account for the "fine structure" of the line spectra. l, designating the angular momentum of an electron in an orbit, indicates the energy difference for "circular" and "elliptical" orbits in the same atom. The values of l are whole numbers ranging from 0 to $(n-1)$.

In a magnetic field the "orbit" of an electron surrounding the atomic nucleus is disturbed in such a way that the vector defining the angular momentum is further "quantized." A third "quantum number," m_l, must therefore be introduced. m_l denotes the projection of the l-axis on the polar axis of the atom. The value of m_l, always given

in whole numbers, may range from $-l$ to $+l$, and may therefore have a total of $2l+1$ different values.

In 1925 Uhlenbeck and Goudsmit found it necessary to introduce the conception of an "electronic spin" in order to account for spectral lines which could not otherwise be explained. This led to the introduction of a fourth "quantum number," m_s, which represents the angular momentum of the "spin" as projected on the polar axis. The value of m_s depends upon the direction of the "spin" and is either $-\frac{1}{2}$ or $+\frac{1}{2}$.

The behavior of every atomic electron in a magnetic field is thus defined by four "quantum numbers." But—and this is a most important "but"—*in the same atom no two electrons can be alike in all four "quantum numbers."* This restriction of the possibilities of electronic behavior, known as Pauli's "exclusion principle," is indispensable to the correct interpretation of spectroscopic phenomena, and is thus one of the basic "principles" of atom physics. Its importance, moreover, reaches far beyond the realm of spectral analysis.

Let us assume that in a given atom a number of electrons have identical values of n and l. They are, in Pauli's terminology, "equivalent" electrons. For them, $2l+1$ different values of m_l are possible, and every such value m_l can be combined with two values of m_s, namely, $-\frac{1}{2}$ and $+\frac{1}{2}$. It follows therefore from Pauli's "exclusion principle" that for any given "quantum number," l, only $4l+2$ "equivalent" electrons are possible. The maximum number of electrons in an atom is therefore

$$\begin{array}{c} 2 \text{ for } l=0 \\ 6 \text{ for } l=1 \\ 10 \text{ for } l=2 \\ 14 \text{ for } l=3 \\ 18 \text{ for } l=4 \end{array}$$

and so on.

Following Arthur Haas's procedure, let us now classify the electrons belonging to a given atom in conformity with their principal "quantum number," n, and let us represent n by a Roman numeral: I, II, III, IV, ... Within each of these groups we arrange sub-groups so that each sub-group is characterized by a specific value of l. Denoting the values of l by Arabic subscripts, we obtain the following scheme of classification:

$$\mathrm{I}_0;\ \mathrm{II}_0,\ \mathrm{II}_1;\ \mathrm{III}_0,\ \mathrm{III}_1,\ \mathrm{III}_2;\ \mathrm{IV}_0,\ \mathrm{IV}_1,\ \mathrm{IV}_2,\ \mathrm{IV}_3;\ \mathrm{V}_0, \ldots.$$

If we now stipulate that, for example, IV_{012} designates an arrange-

ment of electrons in which l has the values of 0, 1, and 2 (but no others), we obtain the following maximum number of electrons for any principal "quantum number," n:

$$\left.\begin{aligned} n_0 &= 2 \\ n_{01} &= 8 \\ n_{012} &= 18 \\ n_{0123} &= 32 \end{aligned}\right\} \qquad (1)$$

That is to say, we obtain numbers which are identical with the numbers of elements in the different "Periods" of the "Table" of elements.

If we now examine the "inert gases" which in each instance complete a "Period," we find that their "atomic numbers" are, respectively, 2, 10, 18, 36, 54, and 86. Utilizing the notation given above, we find that

Helium	$=\mathrm{I}_0$
Neon	$=\mathrm{I}_0+\mathrm{II}_{01}$
Argon	$=\mathrm{I}_0+\mathrm{II}_{01}+\mathrm{III}_{01}$
Krypton	$=\mathrm{I}_0+\mathrm{II}_{01}+\mathrm{III}_{012}+\mathrm{IV}_{01}$
Xenon	$=\mathrm{I}_0+\mathrm{II}_{01}+\mathrm{III}_{012}+\mathrm{IV}_{012}+\mathrm{V}_{01}$
Emanation	$=\mathrm{I}_0+\mathrm{II}_{01}+\mathrm{III}_{012}+\mathrm{IV}_{0123}+\mathrm{V}_{012}+\mathrm{VI}_{01}$

Replacing the symbols by specific numbers in conformity with (1), we get the following values for the inert gases:

Helium	=2	=2
Neon	=2+8	=10
Argon	=2+8+8	=18
Krypton	=2+8+18+8	=36
Xenon	=2+8+18+18+8	=54
Emanation	=2+8+32+18+8	=86.

These values are identical with the "atomic numbers" of the gases in question. Pauli's "exclusion principle" thus furnishes a simple and unitary explanation of the "family resemblances" of the elements within the "Periodic Table" and leads to "electronic patterns" for the individual elements which are identical with the "patterns" of the original Bohr theory. The whole field of sub-atomic structures thus becomes a unitary whole and is integrated through the synthetic power of a single "principle."

Let us now examine the nature of Pauli's "exclusion principle" more closely. In particular, let us examine this "principle" in its

mathematical form (Heisenberg, 1926), for in that form its far-reaching significance is most apparent (cf. Margenau, 1944).

In preceding sections we have seen that a single "particle," whether it is an atom or an electron, can be described by a "wave function," ψ, which is a (continuous) function of the "space variables," x, y, z, and the "spin variable," s. We write therefore: $\psi(xyzs)$.

Under the specific conditions of eigenvalues, we can separate the "spin variable" from the "space variables," $\psi(xyzs) = \chi(xyz) \cdot \sigma(s)$. In order to simplify the discussion, we now neglect the "spin variable" and assume that $\chi(xyz)$ describes the "state" of a single "particle."

When we deal with two independent or non-interacting "particles," A and B, two "wave functions," $\chi_A(x_A y_A z_A)$ and $\chi_B(x_B y_B z_B)$, are needed to describe their respective "states."

If we now view A and B as "belonging together" or as constituting a "system," we must have recourse to an equation involving all six "space variables." In conformity with a well known theorem of the calculus of probability we obtain

$$\psi_{AB}(x_A y_A z_A x_B y_B z_B) = \chi_A(x_A y_A z_A) \cdot \chi_B(x_B y_B z_B). \qquad (2)$$

For *n* independent "particles" viewed as a "system" we have, correspondingly.

$$\psi_n(x_1 \ldots z_n) = \chi_1(x_1 y_1 z_1) \cdot \chi_2(x_2 y_2 z_2) \cdot \ldots \cdot \chi_n(x_n y_n z_n). \qquad (3)$$

The "state function" of a "system" involving nothing but independent "particles" is thus statable as a product of the "states" of the "particles" involved. But when the "particles" comprising the "system" are not independent of each other (if they are, for example, electrons in an atom), then the "state function" of the "system" as a whole is not factorable. Conversely, if the "state function" of a "system" is not factorable, the "particles" comprising the "system" are not independent but "interact."

In dealing with the complex "state functions" describing "systems" of interacting "particles" certain "symmetry operators" must be employed. One of these mathematical "operators" pertains to the possibility of interchanging the coordinates of two "particles." Applying this "operator" to equation (3) would make possible, for example, the transition from the right-hand side of (3) to the statement,

$$\chi_1(x_2 y_2 z_2) \cdot \chi_2(x_1 y_1 z_1) \ldots \cdot \chi_n(x_n y_n z_n).$$

Let us now consider once more a "system" consisting of two "particles," A and B; and let us abbreviate our notation for "state functions" by writing $\chi_A(A)$ for $\chi_1(x_1 y_1 z_1)$, and $\chi_B(B)$ for $\chi_2(x_2 y_2 z_2)$.

Let us denote the "exchange operator" applied to A and B by P_{AB}. We then obtain as "state function" for the "system" as a whole the equation

$$\psi=\chi_A(A)\cdot\chi_B(B);$$

and operator P_{AB} implies that

$$\chi_A(A)\cdot\chi_B(B)=\chi_A(B)\cdot\chi_B(A).$$

The effect of the "exchange operator" upon the "state function" of the "system," $\psi(A,B)$, is normally one of the following three:

(i) $P_{AB}\psi(A,B)$ is a function which is fundamentally different from $\psi(A,B)$; or

(ii) $P_{AB}\psi(A,B)=\psi(A,B)$ (in which case the function is said to be "symmetrical" with respect to the "exchange"); or

(iii) $P_{AB}\psi(A,B)=-\psi(A,B)$ in which case the function is said to be "antisymmetrical").

Pauli's "exclusion principle" stipulates that *only antisymmetrical "state functions" are permissible* (cf. Margenau, Landé).

"Asymmetry" of a function can be obtained only by subtracting the "exchanged" form from the function itself. Hence, if χ_A and χ_B are *different* functions, we obtain

$$\begin{aligned}\psi_{AB}&=\chi_A(A)\chi_B(B)-P_{AB}\chi_A(A)\chi_B(B)\\&=\chi_A(A)\chi_B(B)-\chi_A(B)\chi_B(A)\end{aligned}\tag{4}$$

as the antisymmetrical function corresponding to equation (2); and this function, being no longer a single product, is not factorable in the sense of (2). It implies, therefore, that the "particles," A and B, are not independent of each other, but that they interact. The purely formal requirement of antisymmetry of the functional equation thus entails effects which correspond to those produced by physical "forces." The physicist speaks of these "effects" as the result of "exchange forces."

If the "states" of A and B are *the same*, i.e., if $\chi_A=\chi_B$, the two terms of equation (4) are equal and $\psi_{AB}=0$. This, however, is only another way of saying that in a given "system," ψ_{AB}, the constituent "particles," A and B, cannot be in the same "state," i.e., they cannot be alike in all four "quantum numbers"; if they are alike in all "quantum numbers," the "system" vanishes.

Application of the procedure here indicated to "states" involving *n* "particles" shows that the conclusion just stated holds for all cases, and this conclusion is obviously identical in meaning with the qualitative statement of Pauli's "exclusion principle" as previously given.

In 1927, Heitler and London applied the ideas just outlined to the structure of molecules, interpreting the "bonds" which hold the atoms together as "exchange forces" arising from the antisymmetry imposed upon the "state function" of a given molecule by Pauli's "exclusion principle." This application of the new ideas proved to be so successful that today the whole field of chemistry must be regarded as subsumable under the generalized "laws" of quantum physics (cf. Pauling and Wheland). What this means will become clear from the consideration of one or two of the simplest problems.

For the first example we choose the problem of the "one-electron bond" as exemplified in the hydrogen molecule-ion, H^+_2, which consists of two protons and one electron. In order to determine the "forces" which hold the two protons together we must determine the total energy of the "system," i.e., we must solve the "wave equation" which defines the "state" of this "system." The mathematical difficulties of the "three-body" problem here present no insurmountable obstacle because the mass of the electron is so small compared with the mass of the protons that it can be neglected without seriously impairing the result.

Let us now assume that the two protons constitute fixed centers of force, a distance of r apart. The solution of the "wave equation" then shows that the lowest eigenvalue of energy of the "system," E, is a function of r, and that E becomes a minimum for precisely that value of r which corresponds to the measured distance of the two protons in the ion. In other words, the (quantum mechanical) minimum, E_0, of the eigenvalue of the "system" coincides with the "empirically ascertained" "distance of equilibrium" between H and H^+. Moreover, the calculated value of $-E_0$ agrees well with the "binding energy" of the ion as determined by experiment. Such harmony between "theory" and "observation" cannot be accidental.

But where in all this is the electron? Its position may be inferred when we keep in mind that the square of a "state function" is a "probability index" of the location of a "particle" in space. The value of the square of the "state function" corresponding to E_0 is large in the region between H and H^+ and very small everywhere else. The (negative) electron may therefore be assumed to occupy this place and to provide the "bond" between the two (positive) protons which, in the absence of the electron, repel each other.

It will be remembered, however, that so far we have neglected the "energy value" of the electron. The suggested solution is therefore

at best only a first approximation. But in the case of H^+_2 an absolute solution of the problem is possible; for if one of the protons is infinitely far removed from the other, then this other (together with the electron) is identical with the normal hydrogen atom, and the eigenvalue of that atom can be calculated with precision.

If A is the proton with which the electron is associated, and B is the proton infinitely far away, then the "state function," χ_A, defines this "state" of affairs. But there is no a priori reason why the electron should be associated with A rather than with B. If it is associated with B, A may be regarded as being infinitely far away; and in this case χ_B describes the "state" of affairs. Since both conditions are equally possible, the "state function" of the "system" as a whole can be given as

$$\psi = \chi_A + \chi_B. \tag{5}$$

We must note, however, that under the stipulated conditions this equation holds only if either A or B is at infinity and has no influence upon the "system." Does it still hold when A and B are separated only by a finite distance and when they are interacting members of the "system" H^+_2? It does indeed—provided the functions χ_A and χ_B are written in specific "time dependence" so that the electron may be regarded as "oscillating" between the two protons, now being attached to A, and now to B. This "oscillation" of the electron or (in more recent terminology) this *resonance* between the two "states" accounts for the stability of the hydrogen molecule-ion, for only the "state function" ψ, expressing this condition, yields the minimum or "equilibrium" value of energy for the molecule in question.

The "principle of quantum mechanical resonance" is the assertion that a corresponding interpretation accounts for the stability of *all* molecular structures.

Two reasons prevent us from concluding the discussion at this point: First, the type of "bond" which accounts for the stability of the hydrogen molecule-ion is not typical for all chemical bonds. What is true in this case need, therefore, not be true in any other case. Secondly, our discussion has so far failed to reveal the fundamental connection between quantum mechanical "resonance" and Pauli's "exclusion principle," and until this connection is fully understood, we are not in a position to grasp the full sweep and the extraordinary integrative power of the "exclusion principle." Both deficiencies of our discussion will be eliminated when we consider the next example.

The hydrogen molecule, H_2 (as distinct from the hydrogen molecule-ion), consists of two complete hydrogen atoms and thus involves two electrons as well as two protons. We designate the protons as *A* and *B*, respectively, and the electrons as 1 and 2. We also introduce constant coefficients, a_A and a_B, which we omitted in our first example in order to simplify the discussion. The "state" in which electron 1 is associated with proton A, and electron 2 with proton B can then be expressed by writing $a_A\chi_A(1)\chi_B(2)$. But since it is equally possible that electron 1 is associated with proton *B* and electron 2 with proton *A*, we also have the possibility of a "state" $a_B\chi_B(1)\chi_A(2)$. Allowing for both possibilities, we write the "state function" of the hydrogen molecule in analogy to equation (5) as

$$\psi = a_A\chi_A(1)\chi_B(2) + a_B\chi_B(1)\chi_A(2). \tag{6}$$

The symmetry of this equation implies that a_B and a_A have the same numerical value. According to Pauli's "exclusion principle," however, equation (6) must be antisymmetric with respect to the exchange of the electrons. Hence, if equation (6) were in itself complete, it would entail that $a_B = -a_A$.

So far, however, we have omitted the possible effect of the "electron spin." From the magnetic behavior of the molecule it is evident that the factor representing this "spin" in equation (6) is antisymmetric (cf. Margenau and Setlow). If, therefore, the "electron spin" is taken into consideration, an exchange of electrons 1 and 2 alone is sufficient to change the sign of the function. This means that the rest of the "state function" ψ must be symmetric, and that, therefore, $a_B = +a_A$.

If, under these conditions, the energy value of ψ is calculated and plotted as a function of *r*, the curve has a pronounced minimum for that region of *r* which corresponds to the (experimentally determined) internuclear distance HH, and the energy value at this minimum is in fair agreement with the known "dissociation energy" of the molecule. If one of the constituent "states" of equation (6) is ignored, no such harmony between "theory" and "observation" exists. On the other hand, if ψ^2, as derived from equation (6), is mapped as a function of position in space, its values are found to be largest between the two protons—which is but another way of saying that the "bond" holding the two atoms together may be interpreted as a "piling up" of the negative charges or as a "sharing" of the electrons by the protons in question. The "two-electron bond" of the hydrogen molecule is thus explainable in terms of "exchange forces" which arise from an

application of the "exclusion principle"; it is explainable, in other words, as the result of a quantum mechanical "resonance" between the two "states" defined in equation (6).

The "resonance" interpretation of the "two-electron bond," exemplified here in the case of the hydrogen molecule, H_2, is not restricted to situations in which the atoms held together are of the same kind, for the equivalence of the "states" in equation (6) depends upon the equivalence of the "exchange electrons" rather than upon a fundamental identity of the atoms. This means that the "principle of resonance" leads to satisfactory explanations wherever such "bonds" are found.

The usefulness of the "principle of resonance" is increased still further by the fact that co-valent double and triple bonds can likewise be accounted for in terms of "resonance," and that even the interaction of ionic and co-valent bonds, and the highly important but essentially different "hydrogen bonds" are susceptible to a "resonance" interpretation (cf. Pauling).

The triumph of a quantum mechanical interpretation of chemical phenomena was complete when it became evident that inclusion in the "state function" of the angular distribution of the valence electrons leads to a "resonance" interpretation of directed valence which is in complete agreement with the experimentally ascertained facts. The tetrahedic character of the carbon atom, in particular, could be shown to follow from the "principle of resonance." A single postulate—Pauli's "exclusion principle"—thus provides the logical basis not only for broad fields of physics but for the general field of stereochemistry as well. The unification of physics and chemistry in one all-inclusive system of "laws" has been achieved. (For detailed evidence the reader is referred to the books by Pauling and Wheland mentioned above.)

It may be well to point out that two reasons in particular motivated this lengthy discussion of the "exclusion principle" and of "resonance." First, the far-reaching *importance* of the "principle" could not be demonstrated without presenting at least some of the facts of physics and chemistry for which the "principle" itself provides a satisfactory explanation. Secondly, the *nature* of the "principle" could not be disclosed without revealing its mathematical character and the way it affects the functional equations which describe the "facts" of observation.

The first of these reasons pertains to the integrative power of the "exclusion principle"—a power so amazing that it easily surpasses the

integrative force of any other "principle" in the natural sciences. To the philosopher, however, this integrative power is of secondary importance, for it signifies only another step—although, admittedly, an exceptionally large step—forward in a direction which has long since become evident in the history of science.

The direction of this "integrative process" first became discernible when Newton succeeded in combining Galileo's "law" of falling bodies and Kepler's three "laws" of planetary motion in a more comprehensive "law" of gravitation. It appeared with greater clarity when Einstein joined the realms of mechanics and electromagnetism in a "theory" of relativity, and when he created his "unified field equations" linking electromagnetic phenomena with gravitational fields. And this same "integrative process" is now triumphant in a synthetic sweep which encompasses both physics and chemistry. But in all this, the progress of integration is only a continuation of the basic ideas of integration and synthesis which we first encountered in our interpretation of first-person experience. The unity of the "pattern" which we vaguely anticipated in those earlier discussions as the ideal of every scientific integration of experience has now taken form and shape. We now know that one "principle" suffices to link together in systemic unity the most heterogeneous aspects of broad fields of "observation," and we have new reason to believe that the ultimate goal of all science, as previously defined, may yet be attainable. We shall have more to say on this point in the remaining sections of this book.

Despite its success as an integrative force, Pauli's "exclusion principle" is, and remains, a *postulate* of science. It is neither derivable as a "generalization" from empirical data nor deducible as a "theorem" from the definitions and postulates of mathematics. The restrictions which it imposes upon otherwise permissible mathematical operations are arbitrary, and can be justified only by the results they entail.

As a postulate concerning the formal structure of "state functions," the "principle" itself involves no "picture" or "model" of reality; nor is it a "representation" or "copy" of some "entity" or "thing," or a "law of nature" in the sense in which Boyle's "law" or even the "principle of entropy" may be regarded as such. It is simply a stipulation excluding certain equations from the interdependent system in terms of which we integrate the "pattern" of experience called "nature"; and as such a stipulation it is a tool designed for, and

employed in, the synthetic act of establishing unity and coherence among the variegated phenomena of first-person experience. Its tremendous integrative power is understandable only if we keep in mind (i) the general maxim, first enunciated by Galileo, that in the natural sciences nothing is to be admitted as real save that which can be measured or can be reduced to measure, and (ii) the principle of the "equivalence of matter and energy," which allows us to interpret the whole of reality as a manifestation of "energy." These two ideas together imply the essential unity and homogeneity of all phenomena of the "physical" world. They imply, in other words, that molecular structures and chemical processes are manifestations of "energy" in the same sense in which electromagnetic phenomena are such manifestations; and they imply, furthermore, that as manifestations of "energy," they must be reducible to quantities. Any "state function," therefore, defining a "manifestation of energy" remains within a homogeneous field of quantitative facts so long as it is applied to other "manifestations of energy." The sweeping success of the "exclusion principle" is therefore, in a sense, an indisputable confirmation of the two basic ideas, (i) and (ii), upon which modern science rests. It thus completes the integrative "system" in a (logically) most satisfactory manner. That is to say, as far as physics and chemistry are concerned, the scientific ideal of an integrated and "closed" system has actually been achieved—in principle at least, if not in every detail. As philosophers, we can now turn to problems which transcend this unified realm of the physico-chemical "laws" and can examine a group of facts which may require entirely different principles of interpretation.

The Living Organism

It follows from the "principle of entropy" that all natural processes in the universe occur in such a way that the available energy becomes more and more diffused throughout space, that this process of "degradation" is correlated with the forward "flow" of time, and that it is irreversible. The "principles of causality and uncertainty" imply, furthermore, that certain types of equations constitute a closed system and are indispensable for a strictly scientific integration of experience. Lastly, it is evident from Pauli's "exclusion principle" and from the "principle of quantum mechanical resonance" that the idea of entropy and the "laws" of modern physics are sufficient as means of integration for broad fields of knowledge. The demonstrated interrelation of physics and chemistry proves beyond question that all physico-chem-

ical processes can ultimately be subsumed under identical "laws." But are these "laws" sufficient for an integration of the whole of nature? Do they explain, for example, all facts of observation in the realm of biology? Or do living organisms confront us with phenomena *sui generis* which require a different mode of interpretation? Is the "principle of entropy" or the "principle of quantum mechanical resonance" adequate when we deal with processes of "life," or must we have recourse to a special "vital agent," an "entelechy" or "*élan vital*" which somehow transforms "mere" physico-chemical complexities into "living wholes" and imbues "matter" with the "throbbing heartbeat" of a reality which transcends the reach of quantitative analysis and the domain of entropy?

The old "machine theory" of life, according to which ontogenetic development is but the mechanical unfolding of a minutely differentiated structure of the organism present in the first egg cell, has, of course, been exploded. The "pluripotentialities" of isolated blastomeres, first discovered by Hans Driesch and now proved beyond doubt by countless experiments, entail a complete refutation of all crudely formulated mechanistic theories. But does this mean that no physico-chemical interpretation of "life" is adequate and that the "vitalists" have established their case?

It has never been seriously doubted, I believe, that individual "laws" of physics and chemistry remain in full force when applied to relevant phenomena in living beings. The "laws of the lever," for example, which govern the interrelation of forces and counterforces in lifting a weight, are unchanged whether the "lifting device" is a mechanical "crane" or a "human arm"; and the process of "oxidation" is, chemically speaking, the same kind of a process whether it pertains to the "breathing" of an organism or to the "burning" of a piece of wood. The principles of aerodynamics are as valid for the structure and function of wings and for the "stream-lined" body-forms of birds as they are for the most up-to-date flying machines designed by human engineers. As D'Arcy Thompson has shown, the whole skeleton of a quadruped or a bird, and every part of such a skeleton down to the intrinsic structure of the bones themselves, is related to the "lines of force," the "tensions" and "compressions" and varying "stresses" which it has to encounter in the living animal. The trabeculae, as seen in a longitudinal section of the femur, are thus arranged as "studs" and "braces" along the "lines of stress" in much the same way as that in which an engineer designs an arrangement of "studs" and

"braces" to strengthen a steel structure to withstand a maximum load; and the metacarpal bone of a vulture exemplifies a perfect "Warren's truss" comparable to that used in the construction of the main rib of an airplane wing. From the point of view of an engineer, the quadrupedal backbone with its associated ligaments, membranes, muscles, and tendons can readily be interpreted as a double-armed cantilever girder—different in different species, to be sure, and varying in accordance with the distribution of the load, but subject to the same laws of "tension" and "compression" which govern the construction of the best girders modern engineering skill can design.

The fact, however, that certain laws of physics and chemistry can be applied to specific organic structures and processes does not in itself entail the possibility of a physico-chemical interpretation of "life." On the contrary, the mechanical fitness of organic forms and structures for the particular functions which they perform, and the intricate "self-regulations" which tend to maintain the life and well-being of an organism suggest a "teleological alignment" or "purposiveness" of all "vital" processes which transcends the realm of quantitative analysis. As an energy-system which interacts with the energy pattern of its environment, the living organism maintains itself as a self-adjusting and self-balancing whole. Its manifold processes of adjustment and regulation constitute an intricate and well-integrated system of "equilibration" in which each process contributes to the preservation of the whole, and in which functional arrangements work hand in hand with structural, chemical, and other kinds of arrangements "in order to maintain" life itself and the organism as a living being.

This is not the place for a detailed discussion of the relevant and well-established facts. The experimental work of Claude Bernard, J. S. Haldane, W. B. Cannon, E. F. Adolph, and countless other investigators leaves, however, no doubt in this matter. The living organism is a unitary, self-regulating whole and can be understood fully only if it is considered as such a whole.[2] Any theory, therefore, which disregards the functional integration of the organismic whole is and remains inadequate as an "explanation" of life.

The most difficult as well as the most fundamental question facing the biologist pertains to the origin of the (structurally and functionally) differentiated whole that is a living being. If this question is

[2] For a brief summary of various types of facts relevant to the general problem here under discussion, see Chapter X of *A Philosophy of Science*.

answered satisfactorily in the realm of the sciences, then the solution of all other problems pertaining to biological existence is but an entailment of that answer; but if this question has no satisfactory answer in the realm of science, then our knowledge concerning "life" remains fragmentary and incomplete, and the integrative power of the principles and "laws" of science is materially restricted. It will then be necessary either to revise those "laws" and principles or to have recourse to factors and "forces" which are not reducible to quantitative magnitudes. The issue is crucial for any philosophy of science and, therefore, for philosophy in general.

When Driesch assumed that the first egg cell is but an "aggregate" of highly complex molecules and that the pattern of ontogenetic development, in its origin, is independent of the environment, the logic of the situation demanded recourse to a factor *sui generis*, the "entelechy," which, as a "whole-making" and "directive force," transforms the mere "aggregate" into a functional "whole." As long as Driesch's assumptions are retained, it is logically impossible to avoid "vitalism" or some reference to a "teleological principle." Under these conditions a scientific explanation of "life" is out of the question. This does not mean, however, that a repudiation of Driesch's assumptions is in itself sufficient for a scientific integration of all biological facts, for it is perfectly conceivable that the facts of "life" are essentially such as to defy all attempts at physico-chemical reduction, and that they require the introduction of new types of "laws" and the assumption of new principles of a non-vitalistic nature.

Experimental evidence increasingly indicates that "life" is a continuous "equilibration" in reaction to environmental conditions, and that not only the functional patterns of the mature organism but the developmental patterns in ontogeny as well are highly susceptible to environmental influences. Heredity alone—although providing the potentialities of species character and individual traits—does not determine which of the pluripotentialities of the different regions of a blastula will be realized in actual development. Environmental conditions here play an indispensable part. The chance position of an egg in the ovary, the point of entry of the spermatozoon, and other similar factors produce the various "polarities" which, as a three-dimensional "axiate pattern," constitute the general framework within which embryonic development occurs. Quantitative differences in ontogenetic activity along the several "axes" lead to qualitative differentiations of structure, and thus to the diversified "organs" that are constituent parts of the living whole.

However, sharply defined boundaries between the various "regions" of a developing blastula are usually absent. Instead of such boundaries we commonly find "graded differences" or "gradations" from one region to another. Such "gradients" (as C. M. Child calls them) appear in the protoplasmic structure and physical or chemical constitution of the cells and are discernible in the rate of cell division, in cell size, in growth, morphogenesis, and reconstitution, in the degree of determination of the organs, in respiration, and in reactions to various environmental factors. If such "gradients" are altered or obliterated by experimental interference, the whole pattern of development is changed. This means that the physiological "gradients" are characteristic and essential features of the "axiate order" underlying ontogeny, that, as a matter of fact, the "axiate pattern" is primarily a quantitative gradient pattern within which specific differences of substance and activity arise. "A gradient in a certain direction appears to constitute a physiological basis for definite order and sequence in development in that direction, and a gradient system seems to serve as a sort of a physiological coordinate system with reference to which axiate pattern develops" (Child, pp. 273-274).

As ontogenetic development proceeds, new centers of activity and new gradient systems appear, differing in character from the primary pattern in that they are more specialized and more restricted in potentialities. With increasing differentiation, physiological interrelations between regions become increasingly complex. Specific "integrative factors" become more and more effective and varied. Chemical control or dominance over diversified regions becomes possible and is increasingly important as a factor determining the future course of ontogeny, attaining its greatest influence in the hormone controls of the higher vertebrates—although nervous or neuroid control predominates in most animals.

Some biologists, borrowing a term from the physical sciences, speak of developmental or morphogenetic "fields" as the basis of ontogeny. They refer more specifically to "hydranth fields," "limb fields," "eye fields," or, in general, to the "fields" of specific developmental phenomena.

Of course, the term "field," by itself, although suggesting a solution, does not really solve the problems of ontogeny. It implies the action of an ordering or controlling factor without telling us anything further about its nature or mode of control. But if the idea of a "field" is combined with the conception of physiological "gradients," the terminology is not without significance; for in that case we can think of a

"developmental field" as constituted by the "gradients" present and can regard the "gradients" as the vectors of the "field." Such at least is the essence of Child's theory.

Upon closer inspection, however, we find that even this rather concrete conception of a "field" is not the last word in the matter. Ontogenetic development is primarily an "epigenesis." Structures and forms come into being and are differentiated which were non-existent prior to their development. The specialized organization of the organism is not actually present as basic pattern when ontogeny begins, but emerges as an epigenetic result of the developmental differentiations. This means that the "field" of physiological "gradients" must be interpreted dynamically rather than structurally, as a "field" of action rather than of being. This does not preclude the possibility that in "ontogenetic fields" dynamic and material factors interact in increasingly complex and varied ways, especially at the level of higher vertebrates where manifold hormone and nervous reactions and interactions can be discerned in the functioning of the organic whole; but it does mean that the "structure" of the "field" itself must be understood in terms of dynamic factors which are prior to the observed differentiations. Can such factors be found?

It is Child's contention that the factors in question are found neither in the genes nor the cytoplasmic structures of an undifferentiated egg, but in the varying rates of metabolism which initiate and direct the development of the primary "gradient pattern" and govern the orderly sequence of organic differentiations. The "developmental pattern" appears primarily as an "activity pattern" leading to a definite localization of specifically different substances and thus to morphological structure. In this process the differences in the *rate* of living seem to play a more important part than do differences in the *kind* of living (*ibid.*, p. 164). It is Child's contention, in other words, that in ontogeny the determination and differentiation of organic structures are earlier and later stages of a continuous series of changes, and that the primary pattern which initiates these changes and determines their orderly relation to an "axiate system" is a physiological "gradient field" in which differences in the rate of metabolism constitute the effective and organizing factor.

That Professor Child's contributions to the science of *Entwicklungsmechanik* are far-reaching and important cannot be doubted. His theory of "gradients," combined with the emphasis upon metabolism as a primary factor in ontogeny, indicates an approach and a line

of reasoning of which every scientist will approve. The philosopher has no reason to think lightly of it. But can we rest satisfied with this theory? At least one fundamental question remains still unanswered—the question, namely, What determines the pattern of differential rates in basic metabolism?

If a scientific answer to this question is forthcoming, it is reasonable to expect that it will be given by a biochemist rather than by any other investigator. Much and very significant work pertaining to this problem has already been done, although so far no final solution is in sight. The question is, Can it ever be found in terms of physics and chemistry and in harmony with the principles and "laws" previously discussed?

To my knowledge, the most comprehensive as well as the most authoritative summary of relevant facts in the field of biochemistry is Joseph Needham's formidable volume, *Biochemistry and Morphogenesis*; and to this book the reader is referred for the evidence pertaining to problems here under discussion.

Let us return for a moment to the conception of developmental or morphogenetic "fields," and let us analyze the meaning of a "field" from a slightly different angle. A "field," as now understood and without reference to "gradients," is a "system of order," such that the position of unstable "entities" in one part of the system is definitely correlated with the positions of equally unstable "entities" in other parts. The "field effect" is the result of continuous and varied "equilibrations" of these positions, and ontogeny is a process of transition from a state of instability but high potentiality to a state of stability and restricted potentiality—a process in which the behavior of any given cell is a function of that cell's position in the differentiating whole. Experimental evidence shows that (i) "if a certain amount of material is removed from the domain of a field, the remainder of the field manifests in due course the same pattern which it would normally have given in larger size" (experiments involving the equipotentiality of sea-urchin eggs); (ii) "if unorganized but organizable material is introduced into the field domain, it is incorporated in it" (transplantation experiments); and (iii) "two or more fields can fuse to form a larger one" (fusion experiments on vertebrate eggs) (*ibid.*, 128; cf. Weiss, 1926).

Numerous experiments indicate, furthermore, that the development of such a "field" in the course of ontogeny is determined by two types of interacting factors—specific "organizers" which act upon the mate-

rials or "tissues" of the "field," and the "competence" or capacity of these materials or "tissues" to react in specific ways to the "organizers." When no "organizers" are permitted to act upon it, the presumptive neural plate, for example, possesses no more tendency to turn into neural tissue than does presumptive epidermis. On the other hand, "organizers" with the presumptive fate "head" can induce heads even in the posterior parts of a host body.[3]

The primary "organizer" of a development "field" is located in the mesoderm, for experiments show that the underlying of the dorsal ectoderm by the mesoderm is "the full, perfect and sufficient cause" for the induction of neural differentiation. But what determines or "organizes" the mesoderm? And what is the nature of the "organizers"?

Even if the mesoderm is crushed and boiled, it still organizes, and "coagulated dead pieces of dorsal lip induce neural axes as well as they did when living" (*ibid.*). Furthermore, "organizers" act across genetic boundaries—the "organizers" of one species affecting competent tissue of another. The result, however, is in such cases always determined by the genetic character of the reacting tissue (*ibid.*, pp. 341-351). Lastly, it has been thoroughly established that cell-free extracts as well as a wide variety of chemical fractions and pure or relatively pure substances bring about neural inductions; that they behave, in other words, like primary "organizers" normally found in the dorsal lip at the time of invagination. All of these facts strongly suggest that the "organizers" are chemical substances, that their action upon competent tissue is a chemical action, and that the response to the "organizer action" is a tissue differentiation due to a newly determined direction of the cell metabolism (*ibid.*, p. 119).

We must note, however, that the primary "organizer" found in the dorsal lip is itself a product of an organismic "whole"—a product, namely, of the pattern underlying blastulation. This pattern, in turn, is definitely dependent upon the gene equipment of the first undifferentiated egg cell, for the individual as well as the species specificities of the developing organism are determined by the "gene pattern." "Organizers," therefore, must be regarded as "the intermediary mechanisms between the gene equipment and the final form and properties of the developed animal" (p. 340). Genes, however, according to the generally accepted view, are catalysts, producers of catalysts

[3] For a detailed discussion of "organizers" and "organizer activity," see Needham, *op. cit.*, pp. 97-502.

(enzymes), or producers of "inhibitors." They act in development by "producing, inhibiting the production of, or masking and unmasking, hormones, catalysts, or inhibitors in more or less diffusible states" (p. 418).

It is now definitely known that substances of nuclein type, apart from playing important roles in the cell nucleus, intervene at many previously unsuspected points in the morphogenetic "field." For example, "certain nucleotides in the cytoplasm are essential co-enzymes in glycolysis, phosphorous transfer, hydrogen transfer, and amino-acid oxidation" (p. 631). There is, therefore, increasing evidence that the morphological pattern evolving in ontogeny is an epigenetic resultant of a developmental process which is specifically conditioned by the organized nature of the original cell protoplasm; that the organs, tissues, and other anatomical structures of the mature organism have a counterpart in the "interfaces, oriented catalysts, molecular chains, reaction vessels," and so on, of the living cell (p. 656). As a matter of fact, it has definitely been proved that the micro-morphological arrangement within the cell is an indispensable factor in a variety of metabolic and respiratory phenomena, and that "disorganization of the cell structure in cytolysis destroys some former enzyme-substrate contiguities necessary for the life of the organism, and sets up some new ones, usually not geared to the maintenance of life, nor capable of being so" (p. 657).

Now, if embryonal development is essentially a matter of "chain reactions" induced by successive "organizers," and if the "organizers" themselves are the intermediary mechanisms between "gene equipment" and the final form and structure of the organism, then the earlier a gene exerts its influence the more far-reaching that influence will be. Since, furthermore, ontogeny is a highly integrated process, any change in the character of a gene must of necessity entail a complicated and important modification of the resultant organism—a modification recognizable as a "mutation." Such a change may affect the general pattern of the living whole or the more restricted pattern of some "region" or organ, depending upon the time-rate of effectiveness of the affected gene. The possibility that a single modification of a gene may determine many alterations in the structure of an organism can therefore hardly be denied.

If, in conformity with their catalytic nature, genes can be regarded as complex protein molecules, it is not impossible to conceive a biochemical explanation of mutation; for the change of the gene effecting

the mutation is then perfectly understandable as a transition from one stable form of the protein molecule to another stable but isomeric form of that same molecule. Dehlbrueck, a German physicist, was the first, I believe, to point out this possibility; and Schroedinger, another physicist, has called attention to it in a significant little book, entitled, *What Is Life?* If the conjecture of these men can be shown to be true (and there is much evidence to support their contention), then at least one of the major problems of biological development can be subsumed under the broad principle of quantum mechanical resonance—although the morphological effectiveness of the biochemical change in the gene can be understood only within the general "dynamics" of the developmental "field."[4]

But be that as it may, the fact remains that a protoplasmic complexity, the undeveloped egg cell, as the bearer of organismal differentials, gives rise to "organizers" which, as chemical agents and through chemical action upon "competent" substrata, determine organ development in the embryo. In other words, the fact remains that there is a correspondence of function between the agents which determine the interaction of embryonal "regions" in the developmental "field" and those which govern the autogenous "equilibration" of the adult higher organisms (cf. L. Loeb, Chapter V); and that the mystery of the developing organism is definitely linked up with the chemistry of the living cell. Small wonder, therefore, that embryology is becoming more and more a study of the fibrous protein molecules which constitute cell protoplasm. However, the hope that this may lead to a purely chemical explanation of "life" has already been dimmed. The molecules in question, as the responsible agents in "competence," in cell architecture, and in progressive differentiation, when found in the living body, although not when isolated and purified, are in a state of constant flux and flow. They participate in the emerging structuralization of the embryo, but they participate also in metabolism. Within the living cell, individual atoms are replaced in the molecules with extraordinary speed; and the molecules themselves, no less than the cell structure of which they are parts, form patterns the components of which are in perpetual motion (Needham, p. 677). "Only upon cytolysis and the death of the cell does the characteristic protein curve appear—an indication that some curious structural state is present in the egg" (*ibid.*, p. 656). Hence, while we must admit the importance of protein molecules as guiding factors in embryonic development, we

[4] Cf. *Werkmeister, op. cit.*, pp. 351-353.

are ultimately forced back upon the mystery of how these molecules themselves are formed, determined, and kept in the fluctuating condition which alone is life. A purely chemical interpretation is not the final word.

Let me briefly restate the essential and well-established facts which are relevant to the problem before us.

At the beginning of ontogeny there exists an egg which, in its totipotential nature, is an integrated "whole." It contains all the "directing forces" necessary for the development of a differentiated individual. However, this primary "whole" is not rigidly predetermined as to the actual "fate" of its constituent parts. The epigenetic development which culminates in the mature organism occurs in such a way that the actual fate of each plasmic region depends upon the position of the region within the developing "whole." Normally, this position itself is a function of the developmental "field" associated with the original egg cell. To the extent, therefore, to which this "field" determines the specific fate of the "regions" of the egg, every embryogenetic achievement is the product of an organismic "whole." This "whole" is real and, as a "whole," is more than the sum of its parts. It is harmoniously integrated and functionally adjusted in an uninterrupted process of "equilibration."

The "mechanists" in philosophy insist that this "whole," as a "going concern" in ontogeny, is ultimately explainable in terms of physics and chemistry, and that no new "laws" or principles are involved. The non-mechanists, on the other hand, maintain that the living organism is a special kind of being, that its integrated and functional "wholeness" is inexplicable in terms of physics and chemistry, and that therefore a new approach, new "laws," and new principles are called for.

The issue thus raised involves not only questions of experimental fact, but problems pertaining to the logic of science as well.

In earlier sections of this chapter we have seen that implicit in the nature of the empirical sciences is the ideal of an integrated and closed system. The "mechanists" may now maintain that unless a physico-chemical interpretation of life is accepted, the ideal of the closed system must be abandoned, and science must relinquish its hope of providing a sound basis for a complete integration of all facts of experience. Such an argument, however, is without force.

To begin with, we must carefully distinguish between the ideal of an integrated and closed system as the goal of all science and the conception of such a system as confined to the laws and principles of the

physical sciences. If the facts of observation compel us to transcend the latter, they do not necessarily force us to abandon the former. But even if this were not so, scientific integrity would demand utmost fidelity to the facts of experience regardless of theoretical difficulties. If the facts of experience include—as we know that they do—the specific "wholeness" of living beings, then we must acknowledge this and must adjust our modes of interpretation accordingly; otherwise we turn our "principles of interpretation" into prejudices which blind us to the realities about us. If we begin by defining "life" as nothing but physico-chemical interactions, we deliberately block every avenue which might lead to a more adequate and more comprehensive interpretation of the living "whole"; and such action is not "scientific" in any true sense of that word. After all, each field of inquiry must evolve its own principles and laws and basic categories, and must do so in closest connection with the facts. Each new realm of inquiry requires, if not a new approach, at least an open-mindedness as to its own possibilities and necessities.

I have, of course, no intention of advocating "vitalism" as "the only possible alternative" to a physico-chemical interpretation of life. In previous publications I have explicitly repudiated all forms of "vitalism," and that repudiation remains unchanged. The only achievement of real value which I find in "vitalism" is the negative contention that the phenomena of life are "non-mechanical," i.e., that they cannot be reduced to processes explainable in terms of physics and chemistry.

It is my contention that organic processes are determined and controlled by the uniquely integrated "wholeness" of living cells, that all laws which describe these processes must be formulated with reference to this "wholeness," and that no other interpretation can account for the specific characteristics of living things. I believe, furthermore, that the laws governing the conjoint functionings of the physico-chemical processes in a living organism—i.e., the laws of the organismic "whole"—cannot be derived from the laws of physics and chemistry; for the laws of the living "whole" are of necessity more complex than are the laws of isolated physical or chemical processes, and the more complex laws cannot be derived from the simpler ones by purely logical transformations of the latter. If the unity of science and the ideal of an integrated and closed system are to be preserved, some other mode of dependence or interrelation must be found. And here, I believe, we may take our cue from modern physics.

The theory of relativity strikingly shows how the simpler equa-

tions of Galileo and Newton can be "deduced" from Einstein's more complex equations by the simple device of reducing to zero some of the variables in the Einstein formulae; and quantum mechanics reveals how the laws governing the motions of particles follow from the more intricate laws of wave motions. In every one of theses cases the simpler law is a "limiting condition" of the more complex law and follows from the latter with logical necessity when all complicating factors have either been reduced to zero or have become ineffectual for some other reason. Hence, if a law of the organismic "whole" is ever found, the simpler laws of physics and chemistry will be derivable from it as "special" laws pertaining to those "limiting conditions" when the factors representing the "whole" as such are reduced to zero.

Such an interpretation will preserve the interdependence of scientific laws and, therefore, the ideal of an integrated and closed system. Its logical structure, however, will go counter to all attempts at physico-chemical "reductionism" and "mechanism," and will safeguard the autonomy of every branch of science—and especially the autonomy of biology. It will preserve and be adequate to the uniqueness of the living organism as an integrated "whole." Thoroughly empirical in attitude and approach, this interpretation has nothing in common with fruitless metaphysical speculations, but is in complete harmony with the best of scientific traditions. It is the only interpretation, I believe, which avoids the mysticism and dualism of the "vitalists," and yet is adequate as a scientific view of the living organism as a functional and structural "whole."

We began our discussion of the living organism by enumerating some of the basic principles indispensable to the integrated system of physico-chemical knowledge. We mentioned in particular the "principle of entropy," the "principle of uncertainty," and Pauli's "exclusion principle" or the "principle of quantum mechanical resonance." For our present purpose we can disregard Pauli's "exclusion principle" because it involves only a mathematical convention restricting the acceptable solutions of certain types of equations. The "principle of uncertainty" implies that only laws of an essentially statistical nature can be employed in microphysics; and the "principle of entropy" is itself of a statistical nature, expressing the general tendency of all natural events to increase the amount of unavailable energy in the universe, the tendency of matter to go over from "order" to "disorder." Events can occur and processes can go on only as long as there is a difference in energy potentials between a physico-chem-

ical "system of energy" and its environment. Hence, when a non-living "system" is placed in a uniform environment, then, in time, all motions come to an end, all differences of electric or chemical potentials are equalized, and the temperature becomes uniform throughout. The "system" is ultimately "stabilized" in a thermodynamic equilibrium. Its entropy is the greatest possible, and nothing further can happen without outside interference. But if the "system of energy" placed in that environment is a living organism, the situation is quite different; for by an active interaction with the environment, by eating, drinking, breathing, and assimilating part of its environment the organism maintains itself as a "going concern," sets itself apart in a process of constant "equilibration," and frees itself from the entropy of its surroundings to the increase of which it nevertheless contributes. The living organism, in other words, not only maintains itself as an integrated orderly "whole" but, in embryogeny, it increases its own orderliness in spite of the general deteriorating tendency in nature characterized by an increase in entropy. Any law which is descriptive of the organismic "whole" must, therefore, be formulated in such a way as to express fully this fact of preservation and increase of order, this fact of "*negative* entropy." That is to say, it must be so formulated that only in the special case when the controlling effectiveness of the living "whole" reduces to zero will the "principle of entropy" come into force; and only then will the law of the organismic "whole" yield the ordinary laws of physics and chemistry as "limiting conditions."

We are at present far from having attained this goal. But as a guiding idea in biological research and in science in general it is not without merit, for it indicates the direction in which we must proceed if we are to realize the ideal of an integrated and closed system of scientific knowledge which yet preserves and accounts for the uniqueness of biological existence.

Mind and the Integrated and "Closed" System of Science

Turning now to the human level of existence, we must admit at once that in so far as man is a living organism he is subject to the same conditions and modes of interpretation to which all other living beings are subject. If the type of biological "law" suggested in the preceding section has any significance whatever in the realm of life, it must have the same significance for an amoeba, a starfish, and a human being. Man, however, is not only a *living* being; he is also a *conscious* being—a being having a vast variety of experiences. He is not only an

organism, but a *minded* organism; and this fact changes the whole situation.

Materialists and behaviorists may, of course, take exception to the contention that "mind" makes a difference to the whole scheme of interpretation. The behaviorist, for example, "finds no evidence for mental existence or mental processes of any kind" (Watson); and C. C. Pratt maintains that "within a strict scientific universe of discourse there is no such thing as mind," that "during the early years of the present century it began to lose consciousness under the blows of behaviorism," and that, finally, "at the present time, even its behavior is questionable" and its "very existence is in doubt." This denial of "mind" is, however, by no means as clear and concise in meaning as might be desired. In what sense, for example, is the term "mind" to be understood? If it is meant in the sense of a Cartesian "substance," then there can be no objection to Pratt's contention; but in this case his assertion contains nothing new, for Hume has already denied the existence of "substantial" minds. On the other hand, if Pratt means to assert that there is nothing at all in experience that might appropriately be called "mind," we may not so readily agree with him.

After all, the fact remains that there is "first-person experience," that there is "symbol consciousness," and that a "unitary center of awareness" is the focal point of all "my" experiences. Even Pratt's own views and the views of the materialists and behaviorists have arisen only as the result of reflections upon the content and meaning of first-person experience. Any meaningful assertion—the denial of "mind" included—presupposes all the conditions which are indispensable to meaning and thus presupposes, as a bare minimum, the reality of "awareness" and of "symbol consciousness."

I admit that an interpretation of mind as "awareness" and "symbol consciousness" is in itself inadequate, but it provides at least a definite and secure starting-point from which we may advance to more adequate views.

In Chapter III, in a brief discussion of the "self," we have already pointed out that the "I," as the focal point of "my" first-person experience, is a "self-identical unity"; that it knows itself as such a "unity" despite all changes in the content of experience; that the specific contents of the various "modes of experiencing"—the visual, auditory, tactual impressions, the thoughts and feelings, the memory images and imaginative anticipations—are, in a special sense, elements in a

unitary "biography," and that, as such elements, they are in an equally special sense "private" to that "biography." They are contents of "my" experience. "I" am "aware" of them all.

In the section referred to we found, furthermore, that a "serial" theory of mind cannot readily account for the "unitary self-identity" of the "mind" which persists despite all changes in experienced content, for the "mind" revealed in "my" experience is an enduring, integrating, and purposive agent, not a passing moment in an ever-changing stream or flux of content. We found, in other words, that all the evidence favored, if it did not actually prove, a "substratum" theory of mind.

However, our earlier discussions must now be augmented. In particular, we must consider whether or not the "mind," as the focal point of first-person experience, fits into the interpretative scheme of laws and principles referred to and discussed in the preceding sections. If "mind" should find no place within the integrated and "closed" system of the natural sciences as conceived on the biological level, would a further expansion in the direction already indicated be sufficient, or would it be necessary to open up a new dimension of integration, to form a new system of concepts, principles, and laws which cannot possibly become part of the system of the natural sciences, but "cuts across" it in a unique and irreducible way?

After what has already been said in previous sections about "mind," the factual side of the problem can no longer be in doubt. Any attempt to interpret "mind" as a part of the space-time world of "things" leads into materialism or behaviorism and thus misses the crux of the matter. "Mind," as "symbol consciousness" and as a unitary center of the comprehension of meanings, cannot be understood as mere neural activity or as physiological responses to physico-chemical stimuli, for "meanings" cannot be accounted for in terms of causal relations. "Insight" and "intention," both indispensable to "meaning," are not elements in the world of things.

"Mind," as "symbol consciousness," is a unity of the manifold, an "awareness" of content, and a "being aware" of that "awareness." It is what Kant called a "unity of apperception"; and both ideas, that of "unity" and that of "apperception," are essential to its nature. As *unity* it is the unique center of all objects of first-person experience; but as *awareness* it imposes a restriction upon the experienced content, for the number of objects of which "I" am aware at any given moment is finite and is but a fraction of all possible objects of which

"I" may become "aware." The "span of awareness," furthermore, is such that new objects constantly appear in the range of "my" experience and, having held the center of "attention" for a short time, "fade out" and disappear again. But in the midst of this change the "mind" persists in unitary self-identity.

As the center of reference for all first-person experience, "mind" is not a content of its own experience but, being the focal point of "awareness," stands in irreducible contrast to that content. In this sense, "mind" is that for which everything else is content; and the laws of the natural sciences, therefore, dealing only with content and the interrelations of content of experience, are and remain forever inapplicable to "mind" itself.

This does not preclude the fact, however, that any further characterization and understanding of "mind" can be achieved only through the mind's own content of experience.

In Chapters I and II of this book, "meaning" and "symbol consciousness" were shown to be the basis of all knowledge, and the first principles of integration were discussed. Then, in Chapter III an attempt was made to show how an integration of the contents of first-person experience leads to the conception of an "external" world. The world of "things" was revealed as a pattern of coherent, continuous, and interdependent events. In Chapter IV, and in harmony with the views expressed in Chapter III, "truth" was shown to be essentially a matter of interdependence and coherence of ideas. In other words, in Chapters III and IV the content of first-person experience was interpreted as context, and knowledge of that context turned out to be a "system" of interrelated ideas.

In Chapter V we were concerned with "logic" and with the broad general principles which are the presuppositions of rationality and reasoned understanding. Chapter VI carried this discussion over into the field of mathematics. Here for the first time we encountered the specifically defined ideal of an integrated and "closed" system of concepts, principles, and "laws"—the ideal of a closely knit system of ideas depending upon a few selected definitions and postulates; and we observed the "driving force" of this ideal as motivation in the construction of a "closed" number system.

After a cursory discussion in Chapter VII of various aspects of scientific methodology—a discussion which brought out the difference between a "formal" science and a "natural" science—we examined, in Chapter VIII, the assumptions, principles, and "laws" of physics and

chemistry, and discovered here, in the realm of the natural sciences, the same ideal of an integrated and "closed" system which dominated the field of mathematics, and of which we had caught a first glimpse when we discovered, in Chapter II, that meaning depends upon context. One basic idea has thus permeated all our discussions up to this point.

It must be observed, however, that in all these discussions we have been concerned almost exclusively with the variegated *content* of experience, not with the diversified *"modes" of experiencing* that content. This fact is most obvious in Chapter III, for there we tried to show how the content of first-person experience must be integrated if our belief in an "external" world is to be genuine knowledge and not mere "animal faith"; but inspection will reveal it to be the case in all other chapters as well. We now face the problem of how the elements of that content and the content itself are related to the "mind" which experiences and integrates them.

In the realm of the formal sciences, in logic and mathematics, we were concerned with the interrelation of meanings and the formal interdependence of propositions. The basic relation was that of "entailment." Now, this relation and the type of interdependence it characterizes are completely objective, and in their objectivity is grounded the compelling force of thought in logic as well as in mathematics. Once certain terms have been defined and a few postulates and rules of deduction have been specified, the whole system of "theorems" is rigidly determined in a timeless relation of entailed consequents. Whether some "mind" is aware at any one moment of either a part or the whole of the "system" is immaterial to the nature of the system itself. In this sense at least the "system" is independent of "mind" and is but (actual or possible) content which can enter or leave the "span of awareness" of this "mind" or that. But the *formation* of the system, the *process* of defining the terms, of making the appropriate assumptions, and of actually "deriving" the entailed "theorems"—all this is not a matter of content but of the activity of a "mind." Concept formation, judgment, and reasoning or, in brief, "thinking" is at least one of the essential "modes" of experiencing which characterize "mind" and give concreteness to its nature.

Turning to the realm of the natural sciences—physics, chemistry, biology, and any other branch of knowledge directly concerned with events of the space-time world—we encounter again an objective "pattern" or system which, in its objectivity, is a contextual content

of (actual or possible) experience. In so far as this context is a matter of "laws" and principles, it is related to "mind" in the same way in which the formal systems are related to it; and the mind's "mode" of experiencing is that of "thinking." In the one case as in the other, the definitions, postulates, and rules of deduction are the result of concept formation, judgment, and reasoning, and thus of "thinking." But the integrated and "closed" system of the natural sciences involves manifold and highly diversified elements of an entirely different nature, for it pertains to a type of content of experience which is revealed only in color patterns, sounds, flavors, odors, and other kinds of "perceptual" experiences. To be aware of and to recognize the innumerable sensory qualia which are specific elements of the content of first-person experience is therefore another characteristic aspect of "mind."

But we also know from previous discussions that "perceiving" and "thinking" do not entirely account for all the elements in experience which go into the formulation of the integrated and "closed" system. Thought would be empty without perceptions, but perceptions are unconditionally restricted to the "present" and thought would be so restricted, too, were it not for the fact that the "recall" of past experiences and the "imaginative anticipation" of future events augment the perceptually "given" and thus carry thought far beyond any momentary "present." "Recall" and "imagination" are therefore indispensable "modes" of experiencing content and are as characteristic of "mind" as are "thinking" and "perceiving."

In Chapter III, when we discussed "the reality of other persons," we found that human beings, as existing entities, act upon and are acted upon by various elements of the world of "things"; that they are "agents" pursuing certain "ends" or "goals." We did not discuss this fact at great length at that time, and we need not do so now, for it is generally admitted that in the spheres of "technology," "art," and "morals" man's behavior is "purposive" and is directed toward the attainment of certain "ends." In so far as such "ends" are recognized, defined, and integrated into harmonious "systems of ends," or are related to the "means" necessary to their realization, the "modes" of experience involved are those of "thinking" and "imaginative anticipation" and therefore add nothing to what has already been said. But this "cognitive" attitude toward "ends" is only part of the story. "Ends" are also "evaluated," "desired," and "deliberately pursued" or "willed," and their realization brings "pleasure" or "pain." All of

these "volitional" attitudes and the "feelings" of pleasure and pain are likewise "modes" of experiencing content, and are characteristic of the nature and structure of "mind."

It is apparent from the preceding paragraphs, I believe, that the diversified "modes" of experiencing in which content is related to the "mind" that is aware of it, are only in a secondary sense content for the same "mind," for only an inspection of the primary content *in its relation to the focal point of first-person experience* leads, secondarily, to an understanding of how that content is related to the focal point itself. Any science, therefore, which is concerned with the characteristic features of "mind" as they are revealed in the "modes" of experience deals with a subject matter which is not in the same universe of discourse with the content of experience which is the subject matter of the natural sciences. Hence, to the extent to which psychology is concerned with "mind" in the sense here defined, it indicates a new dimension of investigation and can never be included in the integrated and "closed" system of the natural sciences. The categories of the "external" world, and the principles and "laws" which integrate the *content* of experience are not of the type required for the understanding of the *"modes" of experiencing* that content.

An expansion of the principles and "laws" in the sense in which it was suggested for the realm of biology will thus not do when we are concerned with "mind." The scheme of an integrated and "closed" system, as derived from a study of the natural sciences, breaks down at this point and remains inevitably incomplete and fragmentary. Only the broader and specifically different approach of a general ontology, taking into consideration social living, art, morals, and religion, may yet preserve a semblance of the proud ideal of an integrated "closed" system. But the conception of such a system, carrying us from the secure field of knowledge into the realm of metaphysical speculation, lies outside the scope of this book.

Only one additional point need be mentioned at this time.

Through its connection with the neural pattern of the human organism—as revealed in perception and volition—"mind" is immersed in the space-time world of physical "things." But as the unitary focal point of first-person experience it is neither in space nor in time. Space and time, as experienced spatiality and temporality and as a "constructed" objective space-time order, are content of experience and "forms" of the world of "things" of which "mind" is aware. Moreover, the change in content of the experiencing "mind" is not in itself

a temporal change or a change in any specific time order. The "mind" may range freely from the consideration of a "present" perception to an anticipated "future" goal to a recalled event of the "past," and from there to a contemplation of some aesthetic value or the analysis of a timeless equation of mathematics. Depending in a measure upon the existence of an organism "here and now," "mind" yet ranges without restriction over times and spaces, and its "modes" of experiencing are restricted to no particular "here" or "now." Therein lies its freedom, and therein is rooted the supreme dignity of man—his freedom and value as a moral agent and his autonomy as the lawgiver and judge of his own destiny.

BIBLIOGRAPHY

CHAPTERS I AND II. LANGUAGE AND MEANING

Ajdukiewicz, K., "Sprache und Sinn," *Erkenntnis*, IV (1934).

Aldrich, V. C., "The Spirit of the New Positivism," *Journal of Philosophy*, XXXVII (1940).

Arnold, T. W., *Folklore of Capitalism*, New Haven, 1937.

Ayer, A. J., "A Demonstration of the Impossibility of Metaphysics," *Mind*, XLIII (1934).

Ayer, A. J., *Language, Truth and Logic*, London, 1936.

Ayer, A. J., "The Principle of Verifiability," *Mind*, XLV (1936).

Ayer, A. J., *The Foundations of Empirical Knowledge*, New York, 1940.

Bergmann, G., "Pure Semantics, Sentences, and Propositions," *Mind*, LIII (1944).

Blanshard, B., *The Nature of Thought*, Vol. I, New York, 1940.

Blumberg, A. E., and Boas, G., "Some Remarks in Defense of the Operational Theory of Meaning," *Journal of Philosophy*, XXVIII (1931).

Blumberg, A. E., and Feigl, H., "Logical Positivism," *Journal of Philosophy*, XXVIII (1931).

Britton, K., "Language: Public and Private," *Monist*, XLV (1935).

Britton, K., *Communication: A Philosophical Study of Language*, London, 1939.

Carnap, R., "Die physikalische Sprache als Universalsprache der Wissenschaft," *Erkenntnis*, II (1931).

Carnap, R., *The Unity of Science*, London, 1934.

Carnap, R., *Philosophy and Logical Syntax*, London, 1935.

Carnap, R., *Logical Syntax of Language*, Vienna, 1934; London, 1937.

Carnap, R., "Testability and Meaning," *Philosophy of Science*, III (1936); IV (1937).

Carnap, R., *Foundations of Logic and Mathematics*, International Encyclopedia of Unified Science, I, No. 3.

Carnap, R., *Introduction to Semantics*, Cambridge, Mass., 1942.

Carnap, R., "Hall and Bergmann on Semantics," *Mind*, LIV (1945).

Cassirer, E., *Die Philosophie der Symbolischen Formen*, 3 vols., Berlin, 1923 ff.

Chase, S., *The Tyranny of Words*, New York, 1938.

Cunningham, G. W., "Meaning, Reference, and Significance," *Philosophical Review,* XLVII (1938).

Cunningham, G. W., "On the Linguistic Meaning-Situation," *Philosophy and Phenomenological Research,* IV (1943).

Dalkey, N., "The Limits of Meaning," *Philosophy and Phenomenological Research,* IV (1944).

Dubs, H. H., "Definition and its Problems," *Philosophical Review,* LII (1943).

Ducasse, C. J., "Verification, Verifiability and Meaningfulness," *Journal of Philosophy,* XXXIII (1936).

Ewing, A. C., "Meaninglessness," *Mind,* XLVI (1937).

Fries, H. S., "On an Empirical Criterion of Meaning," *Philosophy of Science,* III (1936).

Gomperz, H., "The Meanings of 'Meaning,'" *Philosophy of Science,* VIII (1941).

Hall, E. W., "The Extra-Linguistic Reference of Language," *Mind,* LII (1943); LIII (1944).

Hartshorne, C., "Metaphysics for Positivists," *Philosophy of Science,* II (1935).

Hayakawa, S. I., *Language in Action,* New York, 1939.

Jespersen, O., *Language: Its Nature, Development, and Origin,* London, New York, 1922.

Jespersen, O., *The Philosophy of Grammar,* London, New York, 1935.

Korzybski, A., *Science and Sanity,* 2nd edition, Lancaster, Pa., 1941.

Lee, D. D., "Conceptual Implications of an Indian Language," *Philosophy of Science,* V (1938).

Lee, I. J., *Language Habits in Human Affairs,* New York, 1941.

Lévy-Bruhl, L., *How Natives Think,* London, 1926.

Lévy-Bruhl, L., *La Mythologie Primitive,* 2nd edition, Paris, 1935.

Lévy-Bruhl, L., *L'Experience et les Symboles chez les Primitifs,* Paris, 1938.

Lewis, C. I., "The Modes of Meaning," *Philosophy and Phenomenological Research,* IV (1943).

Lewis, C. I., "Experience and Meaning," *Philosophical Review,* XLIII (1934).

MacKaye, J., *The Logic of Language,* Hanover, N. H., 1939.

Miller, D. L., "Meaning and Verification," *Philosophical Review,* LII (1943).

Miller, H., "Some Major Confusions of Contemporary Positivism," *Journal of Philosophy,* XXXII (1935).

Morris, C. W., "Philosophy of Science and Science of Philosophy," *Philosophy of Science,* II (1935).

Morris, C. W., *Logical Positivism, Pragmatism, and Scientific Empiricism,* Paris, 1937.

Morris, C. W., *Foundations of the Theory of Signs,* International Encyclopedia of Unified Science, I, No. 2, Chicago, 1938.

Mises, R. von, *Kleines Lehrbuch des Positivismus: Einfuehrung in die empiristische Wissenschaftsauffassung,* The Hague, 1939.

Northrop, F. S. C., "The Significance of Epistemic Correlations in Scientific Method," Reprint from *The Journal of Unified Science (Erkenntnis),* IX (1939).

Ogden, C. K., and Richards, I. A., *The Meaning of Meaning,* 3rd edition, New York, 1930.

Russell, B., *An Inquiry into Meaning and Truth,* New York, 1940.

Schlauch, M., "Semantics as Social Evasion," *Science and Society,* VI (1942).

Schlick, M., "Erleben, Erkennen, Metaphysik," *Kant-Studien,* XXXI (1926).

Schlick, M., "Die Wende der Philosophie," *Erkenntnis,* I (1930-1931).

Schlick, M., "Positivismus und Realismus," *Erkenntnis,* III (1932).

Schlick, M., "Ueber das Fundament der Erkenntnis," *Erkenntnis,* IV (1934).

Schlick, M., "Meaning and Verification," *Philosophical Review,* XLV (1936).

Schlick, M., *Problems of Ethics,* New York, 1939.

Sidgwick, A., "Verifiability and Meaning," *Mind,* XLV (1936).

Springer, O., "Probleme der Bedeutungslehre," *Germanic Review,* XIII (1938).

Stace, W. T., "Metaphysics and Meaning," *Mind,* XLIV (1935).

Stace, W. T., "Positivism," *Mind,* LIII (1944).

Stern, G., *Meaning and Change of Meaning,* Goeteborg, 1931.

Urban, W. M., "Value Propositions and Verifiability," *Journal of Philosophy,* XXXIV (1937).

Urban, W. M., *Language and Reality: The Philosophy of Language and the Principles of Symbolism,* New York, 1939.

Ushenko, A., "The Problem of Semantics," *Journal of Philosophy,* XXXIX (1942).

Vendryes, J., *Language, a Linguistic Introduction to History,* English edition, 1925.

Walpole, H., *Semantics: The Nature of Words and Their Meanings,* New York, 1941.

Weinberg, J. K., *An Examination of Logical Positivism,* New York, 1936.

Werkmeister, W. H., "Seven Theses of Logical Positivism Critically Examined," *Philosophical Review,* XLVI (1937).

Werkmeister, W. H., "The Meaning of 'Meaning' Re-Examined," *Philosophical Review,* XLVII (1938).

Werkmeister, W. H., "Natural Languages as Cultural Indices," *Philosophy of Science*, VI (1939).
Wiener, P. P., "Some Metaphysical Assumptions and Problems of Neo-Positivism," *Journal of Philosophy*, XXXII (1935).
Wisdom, J. O., "Positivism," *Mind*, LIV (1945).

CHAPTER III. THE WORLD ABOUT US

Bauch, B., *Wahrheit, Wert und Wirklichkeit*, Leipzig, 1923.
Blanshard, B., *The Nature of Thought*, Vol. I, New York, 1940.
Brunswik, E., *Wahrnehmung und Gegenstandswelt*, Leipzig, 1934.
Cricker, E. C., *Flavor*, New York, 1945.
Descartes, R., *Meditations* (Eaton edition), New York, 1927.
Drake, D., *et al.*, *Essays in Critical Realism*, New York, 1920.
Ducasse, C. J., "Introspection, Mental Acts, and Sensa," *Mind*, XLV (1936).
Gallie, I., "Is the Self a Substance?" *Mind*, XLV (1936).
Hartmann, N., *Grundzuege einer Metaphysik der Erkenntnis*, 2nd edition, Berlin, 1925.
Hartmann, N., *Der Aufbau der realen Welt*, Berlin, 1940.
Hegel, G. W. F., *The Phenomenology of Mind* (trans. J. B. Baillie), New York, 1931.
Heidegger, M., *Sein und Zeit*, Halle, 1935.
Holt, E. B., *et al.*, *The New Realism*, New York, 1912.
Kant, I., *Kritik der reinen Vernunft* (Schmidt edition), Leipzig, 1924.
Kuelpe, O., *Die Realisierung*, 3 vols., Leipzig, 1912-1923.
Lovejoy, A. O., *The Revolt Against Dualism*, New York, 1930.
Mackay, D. S., "The Illusion of Memory," *Philosophical Review*, LIV (1945).
Montague, W. P., "The Human Soul and the Cosmic Mind," *Mind*, LIV (1945).
Pratt, J. B., *Personal Realism*, New York, 1937.
Price, H. H., *Perception*, New York, 1933.
Santayana, G., *Scepticism and Animal Faith*, New York, 1923.
Schuetz, A., "On Multiple Realities," *Philosophy and Phenomenological Research*, V (1945).
Volkelt, J., *Gewissheit und Wahrheit*, Munich, 1918.
Wood, L., *The Analysis of Knowledge*, Princeton, 1941.

CHAPTER IV. TRUTH

Ayer, A. J., *The Foundations of Empirical Knowledge*, New York, 1940.
Baylis, C. A., "Are Some Propositions Neither True nor False?" *Philosophy of Science*, III (1936).

Baylis, C. A., "Comments on 'Meaning and Truth,'" *Philosophy and Phenomenological Research,* V (1944).

Blanshard, B., *The Nature of Thought,* Vol. II, New York, 1940.

Ducasse, C. J., "Truth, Verifiability, and Propositions About the Future," *Philosophy of Science,* VIII (1941).

Ducasse, C. J., "Propositions, Truth, and the Ultimate Criterion of Truth," *Philosophy and Phenomenological Research,* IV (1944).

Ewing, A. C., "Direct Knowledge and Perceptions," *Mind,* XXXIX (1930).

Hinshaw, V. G., "The Pragmatist Theory of Truth," *Philosophy of Science,* XI (1944).

Joachim, H. H., *The Nature of Truth,* Oxford, 1906.

Kokoszynska, M., "Ueber den absoluten Wahrheitsbegriff und einige andere semantische Begriffe," *Erkenntnis,* VI (1936).

Nagel, E., "Truth and Knowledge of the Truth," *Philosophy and Phenomenological Research,* V (1944).

Neurath, O., "Protokollsaetze," *Erkenntnis,* III (1932).

Parker, DeW. H., "Knowledge by Acquaintance," *Philosophical Review,* LIV (1945).

Reichenbach, H., *Experience and Prediction,* Chicago, 1938.

Sellars, R. W., "A Correspondence Theory of Truth," *Journal of Philosophy,* XXXVIII (1941).

Sellars, R. W., "Meaning of True and False," *Philosophy and Phenomenological Research,* V (1944).

Tarski, A., *The Search for Truth,* Baltimore, 1934.

Tarski, A., "Der Wahrheitsbegriff in den formalisierten Sprachen," *Studia philosophica commentarii societatis philosophicae Polonorum,* I, 1935.

Urban, W. M., "Value Propositions and Verifiability," *Journal of Philosophy,* XXXIV (1937).

Urban, W. M., *Language and Reality: The Philosophy of Language and the Principles of Symbolism,* New York, 1939.

Urban, W. M., "The Dialectic of Meaning and Truth: Truth as Immanent in Discourse," *Philosophy and Phenomenological Research,* IV (1944).

Waters, B., "Basic Sentences and Incorrigibility," *Philosophy of Science,* IX (1942).

Wittgenstein, L., *Tractatus logico-philosophicus,* New York and London, 1922.

CHAPTER V. LOGIC

Ayer, A. J., *Language, Truth and Logic,* London, 1936.

Becker, O., "Zur Logik der Modalitaeten," *Jahrbuch fuer Philosophie und Phaenomenologische Forschung,* II (1930).

Blanshard, B., *The Nature of Thought,* Vol. II, New York, 1940.

Bronstein, D. J., "The Meaning of Implication," *Mind,* XLV (1936).

Carnap, R., *Logical Syntax of Language,* London, 1937.

Carnap, R., *Formalization of Logic,* Cambridge, Mass., 1943.

Cohen, M. R., *A Preface to Logic,* New York, 1944.

Collingwood, R. G., *An Essay on Philosophical Method,* Oxford, 1933.

Cunningham, G. W., "The New Logic and the Old," *Journal of Philosophy,* XXXVI (1939).

Dewey, J., *Logic: The Theory of Inquiry,* New York, 1938.

Dewey, J., *Theory of Valuation,* International Encyclopedia of Unified Science, II, No. 4.

Dewey, J., "Propositions, Warranted Assertability, and Truth," *Journal of Philosophy,* XXXVIII (1941).

Farber, M., "Logical Systems and the Principles of Logic," *Philosophy of Science,* IX (1942).

Goedel, K., "Die Vollstaendigkeit der Axiome des logischen Funktionenkalkuels," *Monatshefte fuer Mathematik und Physik,* XXXVII (1930).

Goedel, K., "Die Vollstaendigkeit der Axiome der *Principia Mathematica,* etc.," *Monatshefte fuer Mathematik und Physik,* XXXVII (1930).

Goedel, K., "Ueber formal unentscheidbare Saetze der *Principia Mathematica* und verwandter Systeme," *ibid.,* XXXVIII (1931).

Goedel, K., "Zum Entscheidungsproblem des logischen Funktionenkalkuels," *ibid.,* XL (1933).

Heyting, A., "Die formalen Regeln der intuistischen Logik," *Sitzungsberichte der Preussischen Akademie der Wissenschaften* (physikalisch-mathematische Klasse), (1930).

Hilbert, D., and Ackermann, W., *Grundzuege der theoretischen Logik,* 2nd edition, Berlin, 1938.

Hofstadter, A., and McKinsey, J. C. C., "On the Logic of Imperatives," *Philosophy of Science,* VI (1939).

Jørgensen, J., "Imperatives and Logic," *Erkenntnis,* VII (1938).

Lewis, C. I., "Alternative Systems of Logic," *Monist,* XLII (1932).

Lewis, C. I., and Langford, C. H., *Symbolic Logic,* New York, 1932.

Lukasiewicz, J., "Philosophische Bemerkungen zum mehrwertigen System des Aussagenkalkuels," *Comptes Rendus des Séances de la Société des Sciences et des Lettres de Varsovie,* Classe III; XXIII (1930), Fascicule 1-3.

Lukasiewicz, J., "Zur Geschichte der Aussagenlogik," *Erkenntnis,* V (1935-36).

Lukasiewicz, J., "Zur vollen dreiwertigen Aussagenlogik," *Erkenntnis,* V (1935-36).

Lukasiewicz, J., and Tarski, A., "Untersuchungen ueber das Aussagenkal-

kuel," *Comptes Rendus des Séances de la Société des Sciences et des Lettres de Varsovie,* Classe III:XXIII (1930).
Menger, K., "The New Logic," *Philosophy of Science,* IV (1937).
Morris, C. R., *The Idealistic Logic,* London, 1933.
Nagel, E., "Intuition, Consistency, and The Excluded Middle," *Journal of Philosophy,* XXVI (1929).
Nagel, E., "Some Theses in the Philosophy of Logic," *Philosophy of Science,* V (1938).
Nelson, E. J., "Intensional Relations," *Mind,* XXXIX (1930).
Nelson, E. J., "Deductive Systems and the Absoluteness of Logic," *Mind,* XLII (1933).
Rougier, L., "The Relativity of Logic," *Philosophy and Phenomenological Research,* II (1941).
Schlick, M., "Erleben, Erkennen, Metaphysik," *Kant-Studien,* XXXI (1926).
Smith, H. B., "The Algebra of Propositions," *Philosophy of Science,* III (1936).
Tarski, A., *The Search for Truth,* Baltimore, 1934.
Tarski, A., "Der Wahrheitsbegriff in den formalisierten Sprachen," *Studia Philosophica,* I (1935).
Tarski, A., "Grundlegung der wissenschaftlichen Semantik," *Actes du congrès international de philosophie scientifique,* Paris, 1936.
Tarski, A., *Introduction to Logic,* New York, 1941.
Weiss, P., "Two-Valued Logic, Another Approach," *Erkenntnis,* II (1928).
Weiss, P., "Relativity in Logic," *Monist,* XXXVIII (1928).
Weiss, P., "On Alternative Logics," *Philosophical Review,* XLII (1933).
Weiss, P., "The Logic of Semantics," *Journal of Philosophy,* XXXIX (1942).
Whitehead, A. N., and Russell, B., *Principia Mathematica,* 3 vols., Cambridge, 1910-13.
Wick, W. A., *Metaphysics and the New Logic,* Chicago, 1942.
Zawirski, Z., "Ueber das Verhaeltnis der mehrwertigen Logik zur Wahrscheinlichkeitsrechnung," *Studia Philosophica,* I (1935).

CHAPTER VI. MATHEMATICS

Ackermann, W., "Begruendung des 'tertium non datur' mittels der Hilbertschen Theorie der Widerspruchsfreiheit," *Mathematische Annalen,* XCIII (1925).
Becker, O., "Beitraege zur phaenomenologischen Begruendung der Geometrie und ihrer physikalischen Anwendungen," *Jahrbuch fuer Philosophie und phaenomenologische Forschung,* VI (1926).
Bell, E. T., *The Development of Mathematics,* New York, 1940.

Bentley, A., *A Linguistic Analysis of Mathematics*, Bloomington, 1932.

Bernays, P., and Schoenfinkel, M., "Zum Entscheidungsproblem der mathematischen Logik," *Mathematische Annalen*, XCIX (1928).

Bernstein, F., "Die Mengenlehre Georg Cantors und der Finitismus," *Jahresbericht der Deutschen Mathematiker-Vereinigung*, XXVIII (1919).

Black, M., *The Nature of Mathematics*, New York, 1934.

Boyer, C. B., *The Concepts of the Calculus*, New York, 1939.

Brouwer, L. E. J., "Intuitionism and Formalism," *Bulletin of the American Mathematical Society*, XX (1914).

Brouwer, L. E. J., "Begruendung der Mengenlehre unabhaengig vom logischen Satz vom ausgeschlossenen Dritten," *Verhandelingen der Koninklijke Akademie van Wetenschappen te Amsterdam, Eerste Sectie*, XII (1918-1919).

Brouwer, L. E. J., "Intuitionistische Mengenlehre," *Jahresbericht der Deutschen Mathematiker-Vereinigung*, XXVIII (1919).

Brouwer, L. E. J., "Zur Begruendung der intuitionistischen Mathematik, I," *Mathematische Annalen*, XCIII (1924).

Brouwer, L. E. J., "Ueber die Bedeutung des Satzes vom ausgeschlossenen Dritten in der Mathematik, insbesondere in der Funktionentheorie," *Journal fuer die reine und angewandte Mathematik*, CLIV (1925).

Brouwer, L. E. J., "Zur Begruendung der intuitionistischen Mathematik, II," *Mathematische Annalen*, XCV (1926).

Cantor, G., *Contributions to the Founding of the Theory of Transfinite Numbers*, Chicago, 1915.

Carslaw, H. S., *Elements of Non-Euclidean Plane Geometry*, London, 1916.

Chwistek, L., "Neue Grundlagen der Logik und Mathematik," *Mathematische Zeitschrift*, 1929, 1932.

Coolidge, J. L., *A History of Geometrical Methods*, Oxford, 1940.

Courant, R., and Robbins, H., *What is Mathematics?*, London, 1941.

Dedekind, R., *Was sind und was sollen die Zahlen?* 1887; 4th edition, Brunswick, 1918.

Dedekind, R., *Stetigkeit und irrationale Zahlen*, 1872; 5th edition, Brunswick, 1918.

Dickson, L. E., *Modern Elementary Theory of Numbers*, Chicago, 1939.

Dresden, A., "Brouwer's Contributions to the Foundations of Mathematics," *Bulletin of the American Mathematical Society*, XXX (1924).

Dresden, A., "Some Philosophical Aspects of Mathematics," *Bulletin of the American Mathematical Society*, XXXIV (1928).

Dubislaw, W., *Die Philosophie der Mathematik in der Gegenwart*, Berlin, 1932.

Fraenkel, A., *Einleitung in die Mengenlehre*, 2nd edition, Berlin, 1923.

Gentzen G., "Die Widerspruchsfreiheit der reinen Zahlentheorie," *Mathematische Annalen,* CXII (1936).

Gentzen, G., "Neue Fassung des Widerspruchsfreiheitsbeweises fuer die reine Zahlentheorie," *Forschungen zur Logik und zur Grundlegung der exakten Wissenschaften,* Neue Folge (1938).

Goedel, K., "Die Vollstaendigkeit der Axiome des logischen Funktionenkalkuels," *Monatshefte fuer Mathematik und Physik,* XXXVII (1930).

Goedel, K., "Die Vollstaendigkeit der Axiome der *Principia Mathematica,* etc.," *Monatshefte fuer Mathematik und Physik,* XXXVII (1930).

Goedel, K., "Ueber formal unentscheidbare Saetze der *Principia Mathematica* und verwandter Systeme," *Monatshefte fuer Mathematik und Physik,* XXXVIII (1931).

Goedel, K., "Zum Entscheidungsproblem des logischen Funktionenkalkuels," *Monatschefte fuer Mathematik und Physik,* XL (1933).

Goedel, K., "Zur intuitionistischen Arithmetik der Zahlentheorie"; "Eine Interpretation des Intuistischen Aussagenkalkuels"; "Zum intuistischen Aussagenkalkuel,"—all in *Ergebnisse eines mathematischen Kolloquiums,* (1933).

Hahn, H., *Logik, Mathematik und Naturerkennen,* Vienna, 1933.

Hardy, G. H., and Wright, E. M., *An Introduction to the Theory of Numbers,* Oxford, 1938.

Heyting, A., "Die formalen Regeln der intuistischen Logik," *Sitzungsberichte der Preussischen Akademie der Wissenschaften* (physikalisch-mathematische Klasse), 1930.

Heyting, A., "Mathematische Grundlagenforschung, Intuitionismus, Beweistheorie," *Ergebnisse der Mathematik,* III (1934).

Hilbert, D., "Axiomatisches Denken," *Mathematische Annalen,* LXXVIII (1918).

Hilbert, D., "Die logischen Grundlagen der Mathematik," *Mathematische Annalen,* LXXXVIII (1923).

Hilbert, D., "Ueber das Unendliche," *Mathematische Annalen,* XCV (1926).

Hilbert, D., "Die Grundlagen der Mathematik," *Abhandlungen des Mathematischen Seminars der Hamburger Universitaet,* VI (1928).

Hilbert, D., *Grundlagen der Geometrie,* 7th edition, Berlin, 1930.

Hilbert, D., "Die Grundlegung der elementaren Zahlenlehre," *Mathematische Annalen,* CIV (1930-1931).

Hilbert, D., and Ackermann, W., *Grundzuege der theoretischen Logik,* Berlin, 1928.

Hilbert, D., and Bernays, P., *Grundlagen der Mathematik,* Berlin, I, 1934; II, 1939.

Hofmann, P., "Das Problem des Satzes vom ausgeschlossenem Dritten," *Kant-Studien,* XXXVI (1931).

Kasner, E., and Newman, J., *Mathematics and the Imagination*, New York, 1940.
Kattsoff, L. O., "Postulational Methods, I and II," *Philosophy of Science*, II (1935).
Kattsoff, L. O., "Postulational Methods, III," *Philosophy of Science*, III (1936) Supplement.
Lesniewski, S., "Grundzuege eines neuen Systems der Grundlagen der Mathematik," *Fundamenta Mathematicae*, XIV (1929).
Lipps, H., "Die Paradoxien der Mengenlehre," *Jahrbuch fuer Philosophie und Phaenomenologische Forschung*, VI (1926).
Nagel, E., "Intuition, Consistency, and The Excluded Middle," *Journal of Philosophy*, XXXVI (1929).
Neumann, J. von, "Zur Hilbertschen Beweistheorie," *Mathematische Zeitschrift*, XXVI (1927).
Pasch, M., "Die Forderung der Entscheidbarkeit," *Jahresbericht der Deutschen Mathematiker-Vereinigung*, XXVII (1918).
Ramsey, F. P., *The Foundations of Mathematics*, London, 1931.
Richardson, M., *Fundamentals of Mathematics*, New York, 1941.
Robinson, G. de B., *The Foundations of Geometry*, Toronto, 1940.
Russell, B., *The Principles of Mathematics*, Cambridge, 1903.
Russell, B., *Introduction to Mathematical Philosophy*, London, 1919.
Toms, E., "The Law of Excluded Middle," *Philosophy of Science*, VIII (1941).
Uspensky, J. V., and Heaslet, M. H., *Elementary Number Theory*, New York, 1939.
Voss, A., *Ueber die mathematische Erkenntnis*, Berlin, 1914.
Waismann, F., *Einfuehrung in das mathematische Denken*, Vienna, 1936.
Werkmeister, W. H., *A Philosophy of Science*, New York, 1940.
Weyl, H., "Ueber die neue Grundlagenkrise der Mathematik," *Mathematische Zeitschrift*, X (1921).
Weyl, H., "Randbemerkungen zu Hauptproblemen der Mathematik," *Mathematische Zeitschrift*, XX (1924).
Weyl, H., *Philosophie der Mathematik und Naturwissenschaften*, Munich, 1926.
Young, J. W., *Lectures on Fundamental Concepts of Algebra and Geometry*, New York, 1923.

CHAPTER VII. SCIENTIFIC METHOD

Alpert, H., "Operational Definitions in Sociology," *American Sociological Review*, III (1938).
Barnes, H. E., "The Historical Philosophy of James Harvey Robinson," *Humanity and Its Problems*, July-August, 1924.

Barnes, H. E., *The New History and the Social Studies,* New York, 1925.

Beard, C. A., "Written History as an Act of Faith," *American Historical Review,* XXXIX (1924).

Benjamin, A. C., "Some Realistic Implications of Operationalism," *Proceedings of the Fifth International Congress of the Unity of Science,* 1939.

Bergmann, G., "Outline of an Empiricist Philosophy of Physics," *American Journal of Physics,* XI (1943).

Bergmann, G., "Frequencies, Probabilities, and Positivism," *Philosophy and Phenomenological Research,* VI (1945).

Bernal, J. G., *The Social Function of Science,* New York, 1939.

Bernard, C., *An Introduction to the Study of Experimental Medicine* (trans. H. C. Greene), New York, 1927.

Bloomfield, L., *Linguistic Aspects of Science,* International Encyclopedia of Unified Science, I, No. 4.

Born, M., *Experiment and Theory in Physics,* Cambridge, 1943.

Boving, E. G., "The Use of Operational Definitions in Science," *Psychological Review,* LII (1945).

Boyle, R., "The Sceptical Chymist," *The Philosophical Works of the Honorable Robert Boyle, Esq.,* III, London, 1738.

Bridgman, P. W., *The Logic of Modern Physics,* New York, 1927.

Bridgman, P. W., *The Nature of Physical Theory,* Princeton, 1936.

Bridgman, P. W., "Science: Public or Private?," *Philosophy of Science,* VII (1940).

Bridgman, P. W., "Some General Principles of Operational Analysis," *Psychological Review,* LII (1945).

Campbell, N. R., *Physics: The Elements,* Cambridge, 1921.

Carnap, R., "On Inductive Logic," *Philosophy of Science,* XII (1945).

Carnap, R., "The Two Concepts of Probability," *Philosophy and Phenomenological Research,* V (1945).

Carnap, R., "Remarks on Induction and Truth," *ibid.,* VI (1946).

Cassirer, E., "The Influence of Language upon the Development of Scientific Thought," *Journal of Philosophy,* XXXIX (1942).

Cohen, M. R., *Reason and Nature,* New York, 1931.

Cohen, M. R., and Nagel, E., *An Introduction to Logic and Scientific Method,* New York, 1934.

Collingwood, R. G., *An Essay on Philosophical Method,* Oxford, 1933.

Cornelius, B. A., "The Operational Theory of Meaning," *Philosophical Review,* XLVI (1937).

Craik, K. J. W., *The Nature of Explanation,* Cambridge, 1943.

Croce, B., *History: Its Theory and Practice,* New York, 1923.

Crowther, J. G., *The Social Relations of Science,* New York, 1941.

Dalton, J., *A New System of Chemical Philosophy,* Manchester, 1808.

Dalton, J., *Foundations of the Atomic Theory,* Chicago, 1911 (Alembic Club Reprints, No. 2).

Destouches, J. L., *Essai sur la forme générale des théories physique, Essai sur l'unité de la physique theorique,* Cluj, Romania, 1938.

Deville, H. St. C., *Leçons sur la dissociation prononcées devant la Société chimique,* Paris, 1866.

Dodd, S. C., "A System of Operationally Defined Concepts for Sociology," *American Sociological Review,* IV (1939).

Dodd, S. C., "A Tension Theory of Societal Action," *American Sociological Review,* III (1939).

Dodd, S. C., *Dimensions of Society: A Quantitative Systematics for the Social Sciences,* New York, 1942.

Dolzhansky, T., *Genetics and the Origin of Species,* New York, 1937.

Dotterer, K. H., "The Operational Test of Meaninglessness," *Monist,* XLIV (1934).

Dubs, H. H., *Rational Induction,* Chicago, 1930.

Dubs, H. H., "The Principle of Insufficient Reason," *Philosophy of Science,* IX (1942).

Ducasse, C. J., "Some Observations Concerning the Nature of Probability," *Journal of Philosophy,* XXXVIII (1941).

Feigl, H., "Operationism and Scientific Method," *Psychological Review,* LII (1945).

Fisher, R. A., "Indeterminism and Natural Selection," *Philosophy of Science,* I (1934).

Fisher, R. A., *The Design of Experiments,* Edinburgh, 1935.

Fling, F. M., *The Writing of History,* New Haven, 1920.

Fortescue, J. W., *The Writing of History,* New York, 1926.

Fourier, J., *The Analytical Theory of Heat* (trans. A. Freeman) Cambridge, 1878.

Frank, P., *Das Kausalgesetz und seine Grenzen,* Vienna, 1930.

Frank, P., *Das Ende der mechanistischen Physik,* Vienna, 1935.

Frank, P., *Between Physics and Philosophy,* Cambridge, Mass., 1941.

Galileo, Galilei, *Dialogues Concerning Two New Sciences* (trans. Crew and de Salvio), New York, 1933.

Gerr, S., "Language and Science," *Philosophy of Science,* IX (1942).

Gilbert, W., *On the Magnet,* London, 1900.

Guilford, J. P., *Psychometric Methods,* New York, 1936.

Haldane, J. B. S., "Quantum Mechanics as a Basis for Philosophy," *Philosophy of Science,* I (1934).

Harvey, W., *An Anatomical Disquisition on the Motion of the Heart and Blood in Animals* (trans. R. Willis), New York, 1907.

Hempel, C. G., "The Function of General Laws in History," *Journal of Philosophy,* XXXIX (1942).

Hempel, C. G., "Studies in the Logic of Confirmation, I, II," *Mind*, LIV (1945).

Hofstadter, A., "Generality and Singularity in Historical Judgment," *Journal of Philosophy*, XLII (1945).

Holzinger, K. J., and Harman, H. H., *Factor Analysis: A Synthesis of Factorial Methods*, Chicago, 1941.

Hook, S., *The Hero in History*, New York, 1943.

Huygens, C., *Treatise on Light* (trans. S. P. Thompson), London, 1912.

Israel, H. E., "Two Difficulties in Operational Thinking," *Psychological Review*, LII (1945).

Jeans, Sir James, *The Universe Around Us*, 4th edition, New York, 1944.

Jeffreys, H., *Theory of Probability*, Oxford, 1939.

Jeffreys, H., "Epistemology and Modern Physics," *The London, Edinburgh and Dublin Philosophical Magazine and Journal of Science*, Seventh Series, No. 212 (1941).

Jepson, W. L., *A Manual of the Flowering Plants of California*, Berkeley, 1925.

Joad, C. E. M., *Philosophical Aspects of Modern Science*, New York, 1932.

Johnson, A., *The Historian and Historical Evidence*, New York, 1934.

Jusseraud, J. A. A. J., *The Writing of History*, New York, 1926.

Kaufmann, F., "The Logical Rules of Scientific Procedure," *Philosophy and Phenomenological Research*, III (1942).

Kaufmann, F., "Verification, Meaning, and Truth," *Philosophy and Phenomenological Research*, IV (1943).

Kaufmann, F., *Methodology of the Social Sciences*, Oxford, 1944.

Kaufmann, F., "Scientific Procedure and Probability," *Philosophy and Phenomenological Research*, VI (1945).

Kaufmann, F., "On the Nature of Inductive Inference," *ibid.*, VI (1946).

Kemble, E. C., "Is the Frequency Theory of Probability Adequate for all Scientific Purposes?," *American Journal of Physics*, X (1942).

Keynes, J. M., *A Treatise on Probability*, London, 1921.

Koehler, W., *Dynamics in Psychology*, New York, 1940.

Lamprecht, K., *What is History?*, New York, 1905.

Langfeld, H. S. (editor), "Symposium on Operationism," *Psychological Review*, LII (1945). "Introduction" and "Rejoinders and Second Thoughts."

Larrabee, H. A., *Reliable Knowledge*, Boston, 1945.

Lenzen, V. F., *Procedures of Empirical Sciences*, International Encyclopedia of Unified Science, I, No. 5.

Lenzen, V. F., "The Concept of Reality in Physical Theory," *Philosophical Review*, LIV (1945).

Lepley, R., "The Verifiability of Different Kinds of Facts and Values," *Philosophy of Science*, VII (1940).

Lindsay, R. B., "A Critique of Operationalism in Physics," *Philosophy of Science*, IV (1937).

Lindsay, R. B., and Margenau, H., *Foundations of Physics*, New York, 1936.

Lundberg, G. A., *Foundations of Sociology*, New York, 1939.

Mandelbaum, M., *The Problem of Historical Knowledge*, New York, 1938.

Margenau, H., "Probability and Causality in Physics," *Monist*, XLII (1932).

Margenau, H., "On the Frequency Theory of Probability," *Philosophy and Phenomenological Research*, VI (1945).

Mayr, E., *Systematics and the Origin of Species*, New York, 1942.

Melton, A. W., "The Logic of Modern Psychology," *Psychological Bulletin*, XXXVIII (1941).

Mendel, G., *Versuche ueber Pflanzenhybriden*, ed. E. Tschermak, Leipzig, 1901.

Mises, R. von, *Wahrscheinlichkeit, Statistik und Wahrheit*, Leipzig, 1928.

Mises, R. von, *Wahrscheinlichkeitsrechnung*, Leipzig, 1931.

Mises, R. von, *Probability, Statistics, and Truth*, New York, 1939.

Morgan, J. J. B., "Credence Given to One Hypothesis Because of the Overthrow of its Rivals," *American Journal of Psychology*, LVIII (1945).

Nagel, E., "A Frequency Theory of Probability," *Journal of Philosophy*, XXX (1933).

Nagel, E., "The Logic of Reduction in the Sciences," *Erkenntnis*, V (1935).

Nagel, E., *Principles of the Theory of Probability*, International Encyclopedia of Unified Science, I, No. 6, Chicago, 1939.

Nagel, E., "Probability and the Theory of Knowledge," *Philosophy of Science*, VI (1939).

Nagel, E., "Operational Analysis as an Instrument for the Critique of Linguistic Signs," *Journal of Philosophy*, XXXIX (1942).

Nagel, E., "Probability and Non-Demonstrative Inference," *Philosophy and Phenomenological Research*, V (1945).

Nagel, E., "Is the Laplacean Theory of Probability Tenable?," *ibid.*, VI (1946).

Neurath, O., "Protokollsaetze," *Erkenntnis*, III (1932).

Neurath, O., "Physicalism: The Philosophy of the Viennese Circle," *Monist*, XLII (1933).

Neurath, O., "Radikaler Physikalismus und 'Wirkliche Welt,'" *Erkenntnis*, IV (1934).

Neurath, O., *Foundations of the Social Sciences,* International Encyclopedia of Unified Science, II, No. 1, Chicago, 1944.

Nevins, A., *The Gateway to History,* Boston, 1938.

Newton, I., *Opticks* (Reprint of 4th edition), New York, 1931.

O'Brien, L., *The Writing of History,* Berkeley, 1935.

Pauling, L. C., and Wilson, E. B., *Introduction to Quantum Mechanics,* New York, 1935.

Pepper, S. C., *World Hypothesis: A Study in Evidence,* Berkeley, 1942.

Pirenne, H., "Stages in the Social History of Capitalism," *The American Historical Review,* XIX (1914).

Planck, M., *Where is Science Going?* (trans. J. Murphy), New York, 1932.

Poincaré, H., *Science and Hypothesis,* New York, 1905.

Popper, K., *Logik der Forschung,* Vienna, 1935.

Porterfield, A. L., *Creative Factors in Scientific Research,* Durham, 1941.

Pratt, C. C., "Operationism in Psychology," *Psychological Review,* LII (1945).

Reichenbach, H., *Wahrscheinlichkeitslehre,* Leiden, 1935.

Reichenbach, H., *Experience and Prediction,* Chicago, 1938.

Reichenbach, H., "Reply to Donald C. Williams' Criticism of the Frequency Theory of Probability," *Philosophy and Phenomenological Research,* V (1945).

Rickert, H., *Die Probleme der Geschichtsphilosophie,* Heidelberg, 1924.

Rickert, H., *Die Grenzen der naturwissenschaftlichen Begriffsbildung,* 5th edition, Tuebingen, 1929.

Robinson, J. H., *The New History,* New York, 1920.

Russell, B., *The Scientific Outlook,* New York, 1931.

Salvemini, G., *Historian and Scientist,* Cambridge, Mass., 1939.

Skinner, B. F., "The Operational Analysis of Psychological Terms," *Psychological Review,* LII (1945).

Stebbing, L. S., *Philosophy and the Physicists,* London, 1937.

Swann, W. F. G., "The Relation of Theory to Experiment in Physics," *Reviews of Modern Physics,* XIII (1941).

Taylor, H., *History as a Science,* London, 1933.

Teggart, F. J., *The Processes of History,* New Haven, 1918.

Thompson, J. W., *A History of Historical Writing,* New York, 1942.

Vincent, J. M., *Aids to Historical Research,* New York, 1934.

Virchow, R., *Cellular Pathology* (trans. F. Chance), Philadelphia, 1863.

Wald, A., "Die Widerspruchsfreiheit des Kollektivbegriffes der Wahrscheinlichkeitsrechnung," *Ergebnisse eines mathematischen Kolloquiums,* Heft 3; also, *Conferences Internationales des Sciences Mathématiques: Les Fondements du Calcul des Probabilités,* 1938.

Weber, C. O., "Valid and Invalid Conceptions of Operationism in Psychology," *Psychological Review,* XLVIII (1942).

Wellmuth, J., *The Nature and Origins of Scientism;* Milwaukee, 1944.

White, M. G., "Probability and Confirmation," *Journal of Philosophy,* XXXVI (1939).

White, M. G., "The Attack on the Historical Method," *Journal of Philosophy,* XLII (1945).

Williams, D., "On the Derivation of Probabilities from Frequencies," *Philosophy and Phenomenological Research,* V (1945).

Williams, D., "The Challenging Situation in the Philosophy of Probability," *Philosophy and Phenomenological Research,* VI (1945).

Williams, D., "The Problem of Probability," *ibid.,* VI (1946).

Wolfle, D. L., *Factor Analysis to 1940,* Chicago, 1940. (This book contains a bibliography of 530 relevant titles, covering the period from 1928 to 1940.)

Zucker, M., *Historical Field Theory, The Philosophy of American History,* 2 vols., New York, 1945.

CHAPTER VIII. SCIENTIFIC CONCEPTS, LAWS, AND PRINCIPLES

Abro, A. d', *The Decline of Mechanism,* New York, 1939.

Adolph, E. F., *Physiological Regulations,* Lancaster, 1943.

Angyal, A., *Foundations for a Science of Personality,* New York, 1941.

Auger, P., *What are Cosmic Rays?,* Chicago, 1945.

Benjamin, A. C., *The Logical Structure of Science,* London, 1936.

Benjamin, A. C., *An Introduction to the Philosophy of Science,* New York, 1937.

Bergmann, G., "An Empiricist Schema of the Psychophysical Problem," *Philosophy of Science,* IX (1942).

Bertalanffy, L. von, and Woodger, J. H., *Modern Theories of Development,* New York, 1933.

Beurlen, K., "Der Zeitbegriff in der modernen Naturwissenschaft und das Kausalprinzip, *Kant-Studien,* XLI (1936).

Birkhoff, G. D., and Lewis, D. C., "Stability in Causal Systems," *Philosophy of Science,* II (1935).

Buehler, K., *Die Krise der Psychologie,* 2nd edition, Jena, 1929.

Burt, C., *The Factors of the Mind: An Introduction to Factor-Analysis in Psychology,* New York, 1941.

Cannon, W. B., *The Wisdom of the Body,* New York, 1932.

Cassirer, E., *Determinismus und Indeterminismus in der modernen Physik,* Goeteborg, 1937.

Child, C. M., *Patterns and Problems of Development,* Chicago, 1941.

Dilthey, W., "Ideen ueber eine beschreibende und zergliedernde Psychologie," *Gesammelte Werke,* V, Leipzig, 1924.

Dirac, P. A. M., *The Principles of Quantum Mechanics,* Oxford, 1930; revised edition, 1935.

Driesch, H., *Grundprobleme der Psychologie*, Leipzig, 1926.

Driesch, H., *The Science and Philosophy of the Organism*, 2nd edition, London, 1929.

Eddington, Sir A. S., *Fundamental Theory*, Cambridge, England, 1946.

Einstein, A., *The Meaning of Relativity*, Princeton, 1945.

Fieser, L. F., and Fieser, M., *Organic Chemistry*, Boston, 1944.

Frank, P., *Das Kausalgesetz und seine Grenzen*, Vienna, 1932.

Frank, P., *Das Ende der mechanistischen Physik*, Vienna, 1935.

Frank, P. "Why Do Scientists and Philosophers so often Disagree about the Merits of a New Theory?," *Reviews of Modern Physics*, XIII (1941).

Goldschmidt, R., *Physiological Genetics*, New York, 1938.

Haas, A., *Quantenchemie*, Leipzig, 1929.

Haas, A., *Materiewellen und Quantenmechanik*, 3rd edition, Leipzig, 1930.

Heisenberg, W., *The Physical Principles of the Quantum Theory*, Chicago, 1930.

Heisenberg, W., *Wandlungen in den Grundlagen der Naturwissenschaft*, Leipzig, 1935.

Holt, E. B., *The Concept of Consciousness*, London, 1914.

Hume, D., *A Treatise of Human Nature*, London, 1739.

Huxley, J. S., and De Beer, G. R., *Elements of Experimental Embryology*, Cambridge, 1934.

James, W., "Does Consciousness Exist?," *Essays in Radical Empiricism*, New York, 1912.

Jeans, Sir J., *Physics and Philosophy*, New York, 1943.

Jeans, Sir J., *The Universe Around Us*, 4th edition, New York, 1944.

Jordon, P., *Physics of the 20th Century*, New York, 1944.

Kaufmann, F., "The Structure of Science," *Journal of Philosophy*, XXXVIII (1941).

Landé, A., *Vorlesungen ueber Wellenmechanik*, Leipzig, 1930.

Lashley, K. S., "The Behavioristic Interpretation of Consciousness," *Psychological Review*, XXX, 1923.

Lenzen, V. F., *The Nature of Physical Theory*, New York, 1931.

Lenzen, V. F., *Physical Causality*, University of California Publications in Philosophy, XV, Berkeley, 1932.

Lillie, R. S., "The Problem of Synthesis in Biology," *Philosophy of Science*, IX (1942).

Lillie, R. S., *General Biology and Philosophy of Organism*, Chicago, 1945.

Loeb, L., *The Biological Basis of Individuality*, Springfield, 1945.

Margenau, H., "Methodology of Modern Physics, I and II," *Philosophy of Science*, II (1935).

Margenau, H., "Critical Points in Modern Physical Theory," *Philosophy of Science*, IV (1937).

Margenau, H., "Metaphysical Elements in Physics," *Reviews of Modern Physics*, XIII (1941).

Margenau, H., "The Exclusion Principle and its Philosophical Importance," *Philosophy of Science*, XI (1944).

Margenau, H., "Phenomenology and Physics," *Philosophy and Phenomenological Research*, V (1944).

Margenau, H., and Setlow, R. B., "Atomic and Molecular Theory since Bohr: Resumé of Specific Results," *American Journal of Physics*, XIII (1945).

Margenau, H., and Wightman, A., "Atomic and Molecular Theory since Bohr: Historical Survey," *American Journal of Physics*, XII (1944).

Margenau, H., and Wightman, A., "Atomic and Molecular Theory since Bohr: Logical and Mathematical Survey," *American Journal of Physics*, XII (1944).

Marshall, H. R., *Consciousness*, New York, 1909.

Mason, P., *The X of Psychology*, Cambridge, Mass., 1940.

Meyerson, E., *Identity and Reality*, New York, 1930.

Moore, J. S., *The Foundations of Psychology*, Princeton, 1921.

Morgan, C. L., *Mind at the Crossways*, London, 1929.

Morris, C. W., *Six Theories of Mind*, Chicago, 1932.

Needham, J., *Order and Life*, New Haven, 1936.

Needham, J., *Biochemistry and Morphogenesis*, Cambridge, 1942.

Neumann, J. von, *Mathematische Grundlagen der Quantenmechanik*, Berlin, 1932.

Pauling, L., *The Nature of the Chemical Bond*, New York, 1939.

Pauling, L., and Wilson, E. B., *Introduction to Quantum Mechanics*, New York, 1935.

Pratt, C. C., *The Logic of Modern Psychology*, New York, 1939.

Ramsperger, A. G., *Philosophies of Science*, New York, 1942.

Rashevsky, N., *Mathematical Biophysics: Physico-Mathematical Foundations of Biology*, Chicago, 1938.

Rashevsky, N., *Advances and Applications of Mathematical Biology*, Chicago, 1940.

Reichenbach, H., *Philosophic Foundations of Quantum Mechanics*, Berkeley, 1944.

Robertson, J. K., *Atomic Artillery and the Atomic Bomb*, New York, 1945.

Schroedinger, E., *What is Life?*, New York, 1945.

Simpson, G. G., *Tempo and Mode in Evolution*, New York, 1944.

Smyth, H. D., *Atomic Energy for Military Purposes*, Princeton, 1945.

Spemann, H., *Embryonic Development and Induction*, New Haven, 1938.

Stern, A. W., "The Neutrino Concept," *Philosophy of Science*, VIII (1941).

Strong, C. A., *Why the Mind Has a Body*, New York, 1903.

Strong, C. A., *Essays on the Natural Origin of the Mind,* London, 1930.
Thompson, D'A. W., *On Growth and Form,* New York, 1942.
Turner, L. A., "Nuclear Fission," *Reviews of Modern Physics,* XII, 1940. (This is a summary of about 100 articles.)
Ushenko, A., "The Problem of Causal Inference," *Philosophy of Science,* IX (1942).
Watson, J. B., *Behavior, an Introduction to Comparative Psychology,* New York, 1914.
Weiss, P., *Morphodynamik,* Berlin, 1926.
Weiss, P., *Principles of Development,* New York, 1939.
Werkmeister, W. H., *A Philosophy of Science,* New York, 1940.
Wheland, G. W., *The Theory of Resonance,* New York, 1944.
Winn, R. B., "Philosophy and Science," *Philosophy of Science,* IX (1942).

INDEX